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JOURNAL

OF THE

FRANKLIN INSTITUTE

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OF THE

State of Pennsylvania

AND

MECHANICS' REGISTER.

DEVOTED TO

MECHANICAL AND PHYSICAL SCIENCE,

CIVIL ENGINEERING, THE ARTS AND MANUFACTURES,

AND THE RECORDING OF

AMERICAN AND OTHER PATENTED INVENTIONS.

EDITED

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NEW SERIES.

VOL. XX.

VOL. 24.

PHILADELPHIA:
PUBLISHED BY THE FRANKLIN INSTITUTE, AT THEIR HALL,
KENNEDY & ELLIOTT, WASHINGTON CITY; G. & C. CARVILL
& CO., NEW YORK; AND JOSEPH H.
FRANCIS, BOSTON.

1837

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JULY, 1837.

Practical and Theoretical Mechanics.

Report of the Committee of the Franklin Institute of the State of Pennsylvania on the Explosions of Steam Boilers, of Experiments made at the request of the Treasury Department of the United States. PART II. *Containing the report of the sub-committee to whom was referred the examination of the strength of the materials employed in the construction of Steam Boilers.*

[CONTINUED FROM PAGE 451.]

Strength of iron made from different sorts of Pig-metal.

The experiments to determine the effect of different kinds of pig-metal, either separate or in mixture, when converted into wrought iron, by the same refining process, were performed on bars furnished by the Salisbury Iron Company, of Salisbury, Connecticut, of which one specimen, from which were formed the two bars 218 A., 218 B., was produced from *dead gray pig;* 219 A., and 219 B., were from a specimen formed from *lively gray pig;* 220 A. and 220 B., from *mottled pig;* 221 A. and 221 B. from *white pig;* and 222 A. and 222 B., from a mixture of all these kinds together. By a reference to the tables (from LXXX. to LXXXVIII. inclusive) containing details of the trials upon these bars, it will be seen that on all of them, some experiments were made at high temperatures, and of course that the purpose of the present comparison can be properly accomplished, only by referring to those, which were made at, or near the same temperature. The experiments at ordinary temperatures embracing the mean strength, as well as the irregularities of structure, are preferred as most satisfactory in referrence to this point. In presenting these results, care has been taken to exclude all those trials in which the effect of heat would be appreciable, either during or subsequent to the time of trial.

It will be seen that if we take into view all the bars of this iron, on which experiments were made after they were reduced to a uniform size, and exclude only 219 B., on which the sections were all deeply filed, the advantage will appear to be in favour of the metal manufactured from *white* pig; next to which, is that produced from *lively gray*, giving 98½ per cent. of the strength of the first. Next, in the order of strength, will be found the iron from *dead gray pig*, inferior to the first by 1 2-3 per cent.; next, that from the mixture of the four kinds of pig which appears to have been weaker than the same by 4 4-10 per cent.; and, finally that from mottled pig in which the inferiority extended to 5 per cent. The following table (LXXIX.) exhibits, at a view, the comparative strength, and the respective degrees of uniformity of the several bars, with the strength of some of them at high temperatures.

At elevated temperatures, the results, except that on No. 219 B, are much nearer to each other, than those at the points selected for our general comparison. On that bar, the trials were upon filed sections. The experiment at 573°, giving a strength of 66620, exceeded those at corresponding temperatures on the other bars, by an average of about 6222 lbs., or 10⅓ per cent.; while the two experiments which were made upon it at low temperatures, as will be seen by table LXXXII., gave results, the mean of which being 66724 lbs., surpasses that of the other nine bars by 9275 lbs., or by 16 1-10 per cent. Hence we are compelled to believe that this specimen, as it came to hand, had undergone the process of hammer-hardening,—a process which the direct experiments of the committee have proved to be capable of essenially modifying the tenacity of the metal.

From the above, it appears, that the greatest difference of strength which under ordinary circumstances, can be attributed to differences in the pig-metal* from which wrought iron is produced, is about 5 per cent., and that under every mode of trial, the article formed from a mixture of different kinds of pig, is inferior in tenacity and uniformity to those derived from either of the ingredients, unless we except that from *mottled gray.* And even this latter will, on a comparison of all the experiments made upon it, under every circumstance, be found superior to the bars from *mixed* castings.

If we take into the amount 219 B., the order of values, beginning with the highest, will be *lively gray*, *white*, *dead gray*, *mixed pigs*, *mottled;* and if we arrange them in the order of their values, as deduced from a comparison of *all the experiments*, on each kind of iron, with the number of trials made on each, we have:—1. *lively gray* 15 experiments.—2. *white* 27 experiments.—3. *mottled gray* 36 experiments.—4. *dead gray* 21 experiments.—5. *mixed metals* 31 experiments.

So far as these experiments may be considered decisive of the question, they favour the lighter complexion of the cast metal, in preference to the darker and mottled varieties, and they place the mixture of different sorts, among the worst modifications of the materials to be used, where the object is mere tenacity.

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* It will be understood that this remark does not apply to pig-iron contaminated with sulphur, phosphorus, copper, or other similar impurities; but only to such as contain different proportions of the *ordinary* ingredients.

TABLE LXXIX.

Comparative table of the effects of using different sorts of pig metal.

No. of the bar.	No. of the expt's. on each bar, made at the comparable temp.	Strength exhibited at each experiment.	Mean Strength of each bar.	Difference of maximum and minimum of each bar.	Amount of irregularity in parts of the mean strength.	Name of the pig, and mean strength of the two bars of each kind.	REMARKS.
218 A.	1	58683				Dead gray pig.	This bar, at 554°, possessed a strength of 60457 lbs. per square inch.
	2	57565					
	10	58459					
	11	61225	59155	4360	.073		
218 B.	1	53314				58016	
	2	55575					
	3	56339					
	4	58682					
	5	60478	56877	7164	.126		
219 A.	1	52257				Lively gray.	This bar at 630° broke with 60010 lbs. per. sq. in.
	2	59418					
	3	60204					
	4	57854					
	8	60645	58075	8388	.144		
219 B.	2	65640				62399	These two experiments were made on very deeply filed sections. At 573°, this bar gave 66620.
	3	67808	66724	2168	.032		
220 A.	1	52503				Mottled.	At 575° broke with 60988 lbs. per sq. inch.
	2	52119					
	3	53999	52873	1496	.028		
220 B.	1	58642				56062	
	2	58108					
	3	58125					
	10	62138	59252	4027	.068		
221 A.	1	53021				White.	At 520° broke under a strain of 60322 lbs. per. sq. inch.
	8	62862					
	9	63081	59654	10060	.168		
221 B.	1	54764				58986	
	2	56054					
	11	57901					
	12	61736					
	13	61140	58319	6972	.119		
222 A.	1	49597				Mixed pigs.	
	2	58924					
	3	60651					
	4	61915					
	5	58895	57996	12318	.212		
222 B.	1	51132				56419	This bar gave as the mean of two experiments at 572° 60215 lbs.
	2	53703					
	3	54407					
	4	59652	54843	8500	.155		

TABLE LXXX.

Experiments on bar 218 A. Manufactured by the Salisbury Iron Company, at Salisbury, in Connecticut, the ore being obtained from "Ore Hill," in that town. The kind of pig used, the "dead gray." Refined after the English method; a loup made and a "gun bar" formed

Marks.	Breadth.	Thickness.	Area at the sections measured before trial.	No. of the experiment.	DATE.	Area of the section of fracture before trial.	Temperature Fahrenheit.	Breaking weight in the scale.	Breaking weight × leverage.	Friction.	Effective strain.
0	.771	.231	.178101		1833.						
1	.755	.231	.174405	1	April 20,	.172895	$5\overset{\circ}{5}6$	356	10680	534	10146
2	.755	.230	.173650								
3	.755	.229	.172895	2	"	.176253	71	356	10680	534	10146
4	.755	.230	.173650								
5	.755	.230	.173650	3	April 25,	.172666	554	366	10980	549	10431
6	.754	.230	.173420								
7	.756	.230	.173880	4	"	.173420	71	397	11910	595	11315
8	.755	.230	.173650								
9	.753	.230	.173190	5	"	.173420	71	397	11910	595	11315
10	.753	.230	.173190								
11	.754	.229	.172666	6	"	.173765	68	388	11640	582	11058
12	.754	.229	.172666								
13	.754	.229	.172666	7	"	.173470	68	396	11880	594	11286
14	.754	.230	.173420								
15	.754	.230	.173420	8	"	.173190	68	404	12120	606	11514
16	.754	.230	.173420								
17	.754	.230	.173420	9	"	.173497	68	392	11760	588	11172
18	.755	.227	.171385								
					Mn. of 9 =	.173611					
Mean of 19			.173512								
Maximum			.173101								
Minimum			.171385								
Mn. of these 2			.174743								
Diff. of the 2			.001716								
Experiments on the specimen as received, filing successively two different sections,											
	Br.	Th.									
	.486	.361		10	April 22,	.153576	50	315	9450	472	8978
	.502	.441		11	"	.221382	68.5	481	14430	721	13709

TABLE LXXX.

expressly for the purpose of affording a specimen for these experiments. The specimen received was drawn out into two bars, A and B, under the hammer, previously to which, however, two sections were filed on the edges and a fracture made at each. Specific gravity 7.7397.

Strength in pounds per square inch.	Point of fracture.	REMARKS.
58683	No. 3	Part in tin from 11½ to 15½. Fracture near the wedges.
57565	" 0½	
60412	" 12¾	Broke in tin.
65246	" 14⅞	Had been in tin at this point.
65246	" 16¾	
63638	" 6¾	
65079	" 8½	
66482	" 9¾	Near the tinned part.
64392	" 5⅔	Had not been in or near the tin. The mean area of the 9 sections of fracture is .000099 square inch greater than that of the 19 sections measured before trial.
and breaking ii at each.		
58459		This section was filed very deep before the bar had been reduced to uniform size.
61925		Filing much less than in the preceding section.

TABLE LXXXI.

Experiments on bar No. 218 *B. Manufactured by the Salisbury Iron Company, at Salisbury, in Connecticut. The ore obtained from "ore hill" in that town. The kind of metal used, "dead gray pig." Refined after the English method.*

Marks.	Breadth.	Thickness.	Areas before fracture.	No. of the exp't.	DATE.	Area of the section of fracture.	Temperature, Fah.	Breaking weight in the scale.	Breaking weight × leverage.	Friction.
0	.763	.227	.173206		1833.					
1	.761	.227	.172747	1	April 27,	.174269	580°	326	9780	489
2	.761	.226	.171986							
3	.761	.227	.172747							
4	.761	.227	.172747	2	"	.174879	96	341	10230	511
5	.761	.227	.172747							
6	.761	.229	.174269							
7	.761	.230	.175030	3	"	.175030	90	346	10380	519
8	.761	.230	.175030							
9	.761	.230	.175030							
10	.761	.230	.175030	4	"	.174840	74.5	360	10800	540
11	.761	.230	.175030							
12	.761	.229	.174269							
13	.761	.230	.175030	5	"	.174840	74	371	11130	556
14	.761	.230	.175030							
15	.762	.229	.174498							
16	.762	.230	.175260	6	May 2,	.175030	77	400	12000	600
17	.761	.229	.174269							
18	.761	.229	.174269							
18.3	.762	.230	.175260	7	"	.172747	77	400	12000	600
		Mean of 20	.174173							
				8	"	.172747	77	400	12000	600
		Maximum	.175260							
		Minimum	.171986							
				9	"	.172747	77	398	11940	597
		Mean of the 2	.173623							
		Diff. of the 2	.003274							
				10	"	.175030	77	398	11940	597
					Mean of 10	.174186				

TABLE LXXXI.

A loup made, and a gun bar formed expressly for the purpose of affording a specimen for these experiments. Drawn under the hammer, reduced by filing, and gauged at every inch from 0 *to* 18 3–10 *inclusive. Specific gravity*, 7.7397.

Effective strain.	Strength in lbs. per square inch.	Point of fracture.	REMARKS.
9291	53314	No. 17½	In tin from 3 to 6½.
9719	55575	" 15½	
9861	56339	" 14	
10260	58682	" 12¾	
10574	60478	" 11¼	
11400	65132	" 10	
11400	65992	" 5	Broke without additional weight.
11400	65992	" 3½	Had been in tin.
11343	65662	" 1	
11343	64806	" 8½	Mean area of 10 sections of fracture is *greater* than that of 20 measured sections by .000013.

TABLE LXXXII.

Experiments on bars No. 219 *A and* 219 *B. Manufactured by the Salisbury Iron Company, at Salisbury, in Connecticut, the ore obtained from "Ore Hill," in that town. The kind of pig used*, lively gray. *Refined after the English method. A loup made and a gun*

Marks. *Bar No.* 219 *A.*	Breadth.	Thickness.	Area of the measured sections before trial.	Marks.	Breadth.	Thickness.	Area after trial.	No. of the experiment.	DATE.	Area of the section of fracture before trial.	Temperature, Fah.	Breaking weight in the scale.	Breaking weight X leverage.
0	.754	.200	.150800										
1	.757	.197	.149129										
2	.759	.198	.150282						1833.				
3	.757	.199	.150643					1	Jan.26,	.145626	63°	267	8010
4	.758	.199	.150842	Meas. after the 9th exper't.									
5	.756	.196	.148176	5	.713	.168	.119784						
6	.754	.196	.147784	6	.713	.187	.133331						
7	.755	.197	.148735	7	.717	.189	.135513	2	"	.147227	50.5	307	9210
8	.755	.197	.148735	8	.722	.190	.137180	3	"	.146286	55.5	309	9270
9	.755	.197	.148735	9	.726	.186	.135036	4	Feb. 2,	.150742	596	306	9180
10	.755	.196	.147980	10	.723	.188	.135924	5	"	.148176	630	312	9360
11	.752	.197	.148144	11	.714	.188	.135232	6	"	.147236	59	324	9720
12	.753	.197	.148341	12	.711	.186	.132246	7	"	.147420	60	341	10230
13	.755	.198	.149490	13	.712	.183	.130296	8	"	.150382	60	320	9600
14	.755	.197	.148735	14	.701	.180	.126180						
15	.756	.196	.148176					9	Feb. 7,	.148176	574	335	10050
16	.756	.195	.147420	16	.670	.175	.117250						
17	.758	.196	.148568	17	.715	.185	.132275	10	"	.149238	54	351	10530
18	.757	.197	.149129	18	.720	.188	.135360						
19	.757	.196	.148372	19	.721	.187	.134827	11	"	.148242	54	355	10650
20	.757	.193	.146101										
21	.756	.192	.145152					12	"	.148735	60	361	10830
22	.756	.195	.147420										
23	.757	.196	.148372					13	"	.148016	65	367	11010
24	.755	.198	.149490										
25	.757	.197	.149129										
26	.754	.194	.146276										
26.4	.753	.196	.147588										
Mean of 28			.148478		Breadth.	Thick.							
Maximum			.150842		.746	.199		1	"	.148454	572	347	10410
Minimum			.145154		.751	.196		2	"	.147196	65.25	339	10170
					.729	.192		3	"	.139968	65	333	9990
Mn. of the 2			.147998										
Diff. of the 2			.005688										

TABLE LXXXII.

bar formed for these experiments. Drawn under the hammer, and bar A. reduced to a uniform size by filing. Bar B. filed deeply at three several points and broken at each filing. Specific gravity 7.8004.

Friction.	Effective strain.	Strength in lbs. per square inch.	Weight producing extension.	Extension measured before the first fracture while the bar was relieved from strain.	Point fractured.	REMARKS.
400	7610	52257	112 203 215 225 235 244 252 260 267	1st percept. .50 in 24 .78 " 1.00 " 1.27 " 1.60 " 1.89 " 2.44 " 2.51 Broke.	No. 20½	Broke at the smallest section in the bar.
460	8750	59418			25⅔	
463	8807	60204			21½	
459	8721	57854			3½	Part in tin from 13½ to 17.
468	8892	60010			15	Broke in tin at the hottest part.
486	9234	62036			19½	
511	9719	65903			16	
480	9120	60645			0¼	
502	9548	64437			5	Broke near the wedges, part in tin from 7½ to 10½.
526	10004	67033			13⅓	Fracture farthest point from the part heated in the last experiment.
532	10118	68253			11½	Part last in tin not yet broken.
541	10289	69175			7⅓	This part was in the packing at the last experiment in hot metal.
550	10460	70668			9⁄6	Had been in tin at 574°. The mean area of the 13 sections of fracture is .000074 square inch *less* than that of the 28 measured sections.
520	9890	66620				Broke in tin.
508	9662	65640				Cold fracture.
499	9491	67808				Do.

TABLE LXXXIII.

Experiments on bar No. 220 A. Manufactured by the Salisbury Iron Company, at Salisbury, in Connecticut. The ore obtained from "Ore Hill," in that town. The kind of pig used, mottled. *Refined after the English method. A loup made, and a gun bar formed*

Marks.	Breadth.	Thickness.	Area before trial.	Marks.	Breadth.	Thickness.	Area after trial.	No. of the experiment.	DATE.	Area of the section of fracture before trial.	Temperature, Fah.	Breaking weight in the scale.
0	.762	.215	.163830	Measured after exp't. 9th.					1833.			
1	.762	.215	.163830	10	.702	.194	.136188	1	Feb. 23,	.163400	69.5°	301
2	.762	.216	.164592	11	.705	.197	.138885					
3	.763	.215	.164045	12	.710	.198	.140580	2	"	.164045	49.5	300
4	.763	.216	.164808	13	.707	.198	.139986					
5	.763	.215	.164045	14	.705	.196	.138180	3	"	.163615	49.5	310
6	.762	.215	.163830	15	.709	.199	.141091					
7	.762	.215	.163830	16	.711	.199	.141489	4	"	.163400	620.	335
8	.760	.215	.163400	17	.716	.200	.143200					
9	.760	.215	.163400	18	.704	.197	.138688	5	"	.163830	650.	363
10	.760	.216	.164160	19	.720	.200	.144000					
11	.762	.216	.164592	20	.721	.200	.144200	6	Feb. 28,	.163830	60.	384
12	.759	.215	.163185	21	.718	.199	.142882					
13	.758	.216	.163728	22	.713	.198	.141174	7	"	.164426	55.	386
14	.759	.216	.163944	23	.706	.196	.138376					
15	.760	.215	.163400	24	.708	.197	.139476	8	"	.164299	55.	386
16	.763	.215	.164045	25	.703	.195	.137085					
17	.762	.214	.163068									
18	.761	.214	.162854	After experiment 12.				9	"	.164211	55.	391
19	.762	.214	.163068	12	.701	.199	.139499					
20	.762	.215	.163830	13	.700	.198	.138600					
21	.762	.215	.163830	14	.696	.197	.137112	10	"	.164448	580.	344
22	.762	.215	.163830	15	.693	.194	.134442					
23	.761	.215	.163615					11	"	.163507	580.	345
24	.760	.215	.163400									
25	.761	.215	.163615	18	.673	.190	.127870	12	"	.163556	575.	350
26	.760	.215	.163400	19	.708	.200	.141600					
27	.762	.215	.163830	20	.710	.198	.140580	13	"	.163780	56.5	354
28	.763	.215	.164045	21	.710	.197	.139870					
29	.763	.215	.164045	22	.704	.201	.141504	14	"	.164376	57.5	354
29.6	.760	.215	.163400	23	.698	.193	.134714	15	"	.163321	58.5	372
								16	Mar. 2,	.163728	59.5	372
		Mean of 31	.163755					17	"	.163782	59.5	372
								18	"	.163400	60.5	368
		Maximum	.164808					19	"	.163776	60.5	368
		Minimum	.162854					20	"	.163322	61.	381
								21	"	.162907	61.	381
		Mn. of these 2	.163831									
									Mean of 21	.163755		
		Diff. of the 2	.001954									

TABLE LXXXIII.

expressly for these experiments. Drawn under the hammer, reduced by filing, and gauged at every inch from 0 *to* 26.6, *inclusive. Specific gravity*, 7.7855.

Weight × leverage.	Friction.	Effective strain.	Strength in pounds per square inch.	Point of fracture.	REMARKS.
9030	451	8579	52503	No. 9	
9000	450	8550	52119	" 29	A part not included in the first experiment, the bar being too long to be taken all at once into the machine.
9300	465	8835	53999	" 26½	
10050	502	9548	58433	" 8	Part in tin from 3½ to 6. After this experiment, sent the bar forward and took hold again, without changing the tin.
10890	544	10346	63151	" 7	Broke at the coldest part.
11520	576	10944	66801	" 6	
11580	579	11001	66905	" 4½	
11580	579	11001	66958	" 3⅓	
11730	586	11144	67864	" 1½	This section of the bar is now finished. Part from 9 to 26½ was then gauged again. This section had not been in tin.
10320	516	9804	59618	" 10⅔	Broke near the wedges. Part in tin from 15 to 18½.
10350	517	9833	60138	" 24½	Broke near the wedges.
10500	525	9975	60988	" 16½	Broke *in the tin.*
10620	531	10089	61601	" 9½	Within the gripe of the wedges.
10620	531	10089	61378	" 10½	Do. do.
11160	558	10602	64915	" 12¼	
11160	558	10602	64754	" 13	
11160	558	10602	64610	" 13¾	Near the wedges.
11040	552	10488	64186	" 24	Remote from tinned part.
11040	552	10488	64039	" 22¼	
11430	571	10859	66488	" 19⅓	
11430	571	10859	66658	" 18¼	
					The mean area of the 21 sections of fracture is *identical* with that of the 31 measured sections.

TABLE LXXXIV.

Experiments on bar No. 220 *B. Manufactured by the Salisbury Iron Company, at Salisbury, Connecticut. The ore obtained from "Ore Hill," in that town. The kind of pig employed, the* mottled. *Refined after the English method. A loup made and a gun bar formed*

Marks.	Breadth before trial.	Thickness before trial.	Area before trial.	Marks.	Breadth after trial.	Thickness after trial.	Area after trial.	No. of the exp't.	DATE.	Areas of section of fracture before trial.	Temperature, Fah.	Breaking weight in the scale.	Breaking weight × leverage.
0	.752	.220	.165440	Measures taken after the first experiment.									
1	.756	.218	.164808						1833.				
2	.756	.218	.164808	No. 5	.697	.199	.138703	1	Feb. 21,	.163296	63.5°	336	10080
3	.756	.216	.163296	Smallest at beginning.									
4	.756	.217	.164052	8	.719	.208	.148833						
5	.752	.217	.163184	Largest at beginning.									
6	.754	.217	.163618	24¾	.697	.197	.137309						
7	.757	.219	.165783	Smallest section at present.				2	"	.164797	575	336	10080
8	.754	.218	.164372										
9	.756	.218	.164808										
10	.756	.218	.164808					3	"	.164749	62.5	336	10080
11	.753	.218	.164154										
12	.754	.218	.164372					4	"	.164749	576	347	10410
13	.754	.218	.164372										
14	.754	.218	.164372					5	"	.164907	572	361	10830
15	.756	.218	.164808					6	"	.164749	580	371	11130
16	.754	.218	.164372					7	"	.164530	67	389	11670
17	.754	.218	.164372					8	Feb. 23,	.164907	67	394	11820
18	.754	.219	.165126					9	"	.165126	67	397	11910
19	.754	.218	.164372	Measurements taken after the sixth experiment.				10	"	.164797	67	397	11910
20	.754	.219	.165126										
21	.754	.218	.164372	21	.682	.196	.133672	11	"	.165124	71.5	360	10800
22	.754	.219	.165126	22	.697	.200	.139400						
23	.754	.219	.165126	23	.677	.196	.132692	12	"	.163329	550	364	10920
24	.752	.219	.164688	24	.665	.191	.127015						
25	.753	.219	.164907	25	.668	.193	.128924	13	"	.164808	590	364	10920
26	.753	.219	.164907	26	.707	.200	.141400						
27	.753	.218	.164154	27	.717	.206	.147702	14	"	.165077	61	388	11640
28	.753	.219	.164907	28	.713	.203	.144739	15	"	.164481	61	371	11130
29	.753	.219	.164907					16	"	.164590	61.25	385	11550
30	.752	.219	.164688					17	"	.164372	61.25	385	11550
Mean of 27			.164587					18	"	.164372	61.25	393	11790
Maximum			.165783						Mn. of 18	.164598			
Minimum			.173184										
Mn. of the 2			.164483										
Diff. of the 2			.002599										

TABLE LXXXIV.

expressly for these experiments. Drawn under the hammer, reduced by filing to a nearly uniform size and gauged at every inch, from 0 *to* 30. *Specific gravity*, 7.7855.

Friction.	Effective strain.	Strength in pounds per square inch.	Weights producing elongation.	Elongation after being relieved from each wht.	Point of fracture.	REMARKS.
504	9576	58642	180 184 284 289 336	1st perman. 1-40 in. in 24 24 bec. 25.5 " 27. Broke.	No. 3	The bar was too long to be all put in. Took the part between 0 and 24 being the most uniform—the first elongation was under 23-48 of the breaking weight.—Very uniformly stretched.
504	9576	58108			29½	In tin from 23½ to 26½. Broke near the wedges,—this part not strained in the first experiment. Broke very soon under the same weight as before—probably would have broken with a little less.
504	9576	58125			17½	Weight put on gradually.
520	9890	60030			19½	From 22½ to 25½ now in tin. Broke near the wedges.
541	10289	62393			28½	Fracture at the opposite end to the preceding.
556	10574	64182			20½	Broke at the opposite end to the preceding.
583	11087	67386			27½	Opposite end to the preceding.
591	11229	68093			26	Same end as the preceding.
595	11315	68523			22	Opposite end to the preceding.
595	11315	68660			24½	This exper. finished this part of the bar.
540	10260	62135			0½	Not heated. Piece broken off in first exp.
546	10374	63516			5⅓	Piece now in from 3 to 17½. Part in tin from 8 to 12. Greatest wt previously borne 336.
546	10374	62946			10	Broke in tin with the same weight as it bore in the preceding experiment.
582	11058	66987			7½	Part now tried from 5⅓ to 10.
556	10574	64287			10½	Part now in from 10 to 17½.
577	10973	66669			15½	Do. from 10½ to 17½.
577	10973	66757			13½	Do. " 10½ to 15½.
589	11201	68144			12½	Do. " 10½ to 13½.
						The mean area of 18 sections of fracture is .000011 square inch *greater* than that of the 31 measured sections.

TABLE LXXXV.

Experiments on bar No. 221 *A. Manufactured by the Salisbury Iron Company, at Salisbury, in Connecticut. The ore obtained from* " *Ore Hill,*" *in that town. The kind of* metal *employed,* " white pig," —*refined after the English method. A loup made and a gun bar*

Marks.	Breadth.	Thickness.	Areas before trial.	No. of the exp't.	DATE.	Area of the section of fracture before trial.	Temperature, Fah.	Breaking weight in the scale.	Breaking weight × leverage.	Friction.	Effective strain.
1	.757	.224	.169568		1833.						
2	.752	.227	.170704	1	Mar. 14,	.170931	570.°	318	9540	477	9063
3	.754	.229	.172666								
4	.752	.228	.171456	2	"	.172060	580.	381	11430	571	10859
5	.753	.229	.172437								
6	.753	.229	.172437								
7	.753	.228	.171684	3	"	.171031	520.	362	10860	543	10317
8	.753	.228	.171684								
9	.753	.229	.172437								
10	.753	.229	.172437	4	"	.172186	73.	404	12120	606	11514
11	.753	.227	.170931								
12	.753	.227	.170931	5	"	.172437	72.	404	12120	606	11514
13	.752	.227	.170704								
14	.753	.228	.171684	6	"	.169947	71.5	404	12120	606	11514
15	.753	.228	.171684								
16	.753	.227	.170931	7	"	.171456	71.	404	12120	606	11514
17	.751	.228	.171228								
18	.752	.227	.170704	8	Mar. 16,	.170931	78.5	377	11310	565	10745
19	.753	.227	.170931								
20	.753	.227	.170931	9	"	.171684	78.	380	11400	570	10830
21	.753	.227	.170931								
22	.753	.227	.170931								
23	.753	.227	.170931	10	"	.170931	78.	380	11400	570	10830
24	.753	.228	.171684								
25	.753	.228	.171684								
$25\frac{6}{10}$	.752	.228	.171456	11	"	.170931	77.	392	11760	588	11172
Mean of 26=			.171376	12	"	.170817	77.	392	11760	588	11172
Maximum=			.172666	13	"	.170966	76.5	401	12030	601	11429
Minimum =			.169568								
				14	"	.179931	76.	394	11820	591	11229
Mn. of these 2			.171117								
					Mean of 14	.171231					
Diff. of the 2			.003098								

TABLE LXXXV.

formed expressly for these experiments. Drawn under the hammer, subsequently reduced by filing to a nearly uniform size, and gauged at every inch. Specific gravity, 7.8018.

Strength in lbs. per square inch.	Point of fracture.	REMARKS.
53021	No. 21	Part in tin from 2½ to 5½.
63111	" 8½	Same part in tin. Temperature had been as high as 600° for a short time.
60322	" 13⅓	Broke in tin. Part immersed from 13 to 16, inclusive. Had been strained in experiment No. 1. Temperature had been 550°.
66869	" 6⅓	Part left in experiment 2d., which did not break *in tin.* Now broke at a part which was of a straw colour.
66772	" 5	Broke with the same weight as above.
67750	" 1⅓	Bore the weight a moment, and then gave way.
67154	" 4	Bore the weight a short time.
62862	" 22¾	This part was broken off in experiment No. 1.
63081	" 25	Do.
63358	" 19	The weight is supposed to have been too great. The whole weight employed in the last experiment put on by mistake. The bar broke immediately.
65359	" 20	
65403	" 18½	Gradually gave way under the same weight as the preceding.
66849	" 17¼	
65693	" 11½	A different piece from the preceding.
		The mean area of the 14 sections of fracture is .000145 square inch *less* than that of the 26 measured sections.

TABLE LXXXVI.

Experiments on bar No. 221, B. Manufactured by the Salisbury Iron Company, at Salisbury in Connecticut. The ore obtained from "Ore Hill" in that town. "White pig" *metal, refined after the English method,—a loup formed and a "gun*

Marks.	Breadth.	Thickness.	Area at the sections measured before trial.		No. of the experiment.	DATE.	Area of the section of fracture before trial.	Temp. Fah.	Breaking wht. in the scale.	Breaking weight × leverage.	Friction.	Effective strain.
1	.762	.232	.176784			1833.						
2	.753	.229	.172437		1	Mar. 21,	.174347	57$\overset{\circ}{6}$	335	10050	502	9548
3	.753	.229	.172437									
4	.753	.229	.172437		2	"	.173387	574	341	10230	511	9719
5	.754	.229	.172666									
6	.753	.230	.173190		3	"	.172437	576	378	11340	567	10773
7	.754	.231	.174174									
8	.755	.231	.174405									
9	.754	.229	.172666									
10	.755	.229	.172895		4	Mar. 23,	.173420	576	392	11760	583	11172
11	.755	.230	.173650									
12	.755	.230	.173650		5	"	.173420	576	402	12060	603	11457
13	.755	.230	.173650									
14	.754	.230	.173420									
15	.754	.230	.173420		6	"	.173928	100	410	12300	615	11685
16	.755	.230	.173650									
17	.755	.230	.173650									
18	.754	.230	.173420		7	"	.174289	90	416	12480	624	11856
19	.755	.230	.173650									
20	.756	.229	.173124		8	"	.173101	68	425	12750	637	12113
21	.755	.230	.173650									
22	.755	.231	.174405		9	"	.173147	68	425	12750	637	12113
23	.755	.231	.174405	Unfiled portion near the end.								
24	.754	.231	.174174		10	"	.173650	68	425	12750	637	12113
25	.752	.232	.174464	Br. Th.								
25.6	.755	.232	.175160	.850 | .227	11	"	.192950	68	392	11760	588	11172
Mean of 26			.173682									
					12	"	.173124	67	375	11250	562	10688
Maximum			.176784									
Minimum			.172437									
					13	"	.174812	66	375	11250	562	10688
Mean of the 2			.174610									
Diff. of the 2			.004347			Mn. of 12 =	.173589					

TABLE LXXXVI.

bar" drawn expressly for these experiments. Drawn under the hammer into two bars, each reduced by filing to a uniform size, and then marked and gauged at every inch. Specific gravity, 7.8018.

Strength in lbs. per sq. inch.	Point of fracture.	REMARKS.
54764	No. 23¼	Part in tin from 3 to 6½.
56054	" 19½	Same part still in tin.
62475	" 4	Took hold of the bar near the pan of melted metal, leaving a part for future experiment, and compelling it to break *in* or near the hottest part. Fracture *within the tin.*
64421	" 18	Part in tin from 12 to 15½ which had been tried in the second experiment—fracture near the end.
66065	" 14¼	Broke *in the tin.*
67183	" 6¾	The part now put in is from 4 to 14, both ends of which have been broken in tin at 576°. Fracture took place outside of where it had been in the tin.
68025	" 7½	Broke at a part which had been less heated than the preceding.
69976	" 8¾	Had not been in tin.
69958	" 10⅓	Had been near the tin.
69755	" 11½	Do. This part of the specimen is now finished.
57901	" unc.	Part now in the machine is from 1 to 4. The *unfiled part* being in the wedges, the fracture took place at that part.
61736	" 20	Short piece.—Had been between two former cold fractures in experiment second.
61140	" 25⅓	This piece had likewise been tried only at a cold fracture in experiment first. No additional weight was required. The mean area of the 12 sections of fracture on the filed part was .000093 square inch *less* than that of the 26 measured sections.

TABLE LXXXVII.

Experiments on bar No. 222 A. Manufactured by the Salisbury Iron Company, at Salisbury in Connecticut. The ore obtained from "Ore Hill," in that town. The metal, a mixture of "dead gray," "lively gray," "mottled," and "white" pigs, refined after the English method, a loup formed and a gun bar drawn expressly for these experi-

Marks.	Breadth before trial.	Thickness before trial.	Areas of the measured sections before trial.	Marks.	Breadths after trial.	Thickness after trial.	Areas after trial.	No. of the experiment.	DATE.	Area of the section of fracture.	Temperature, Fahrenheit.	Breaking weight in the scale.
0	.766	.221	.169286									
1	.759	.219	.166221						1833.			
2	.759	.219	.166221					1	Mar. 21,	.168942	560°	294
3	.761	.218	.165898									
4	.762	.219	.166878									
5	.762	.220	.167640	Measures taken after the 4th experiment.								
6	.761	.221	.168181					2	"	.169286	73.5	350
7	.762	.222	.169164	1	.728	.209	.152252					
8	.764	.222	.169608	2	.735	.207	.162145					
9	.762	.222	.169164	3	.730	.209	.152570					
10	.762	.222	.169164	4	.726	.207	.150282	3	"	.169164	73.5	360
11	.759	.222	.168498	5	.694	.196	.136024					
12	.761	.222	.168942	6	.690	.200	.138000	4	"	.168942	73	367
13	.761	.222	.168942	6½	.675	.193	.130275					
14	.762	.222	.169164	7	.690	.199	.137310	5	"	.168402	572	348
15	.763	.222	.169386	8	.731	.207	.151317					
16	.762	.222	.169164	9	.725	.210	.152250	6	Mar. 23,	.169164	75	377
17	.761	.222	.168942	10	.730	.210	.153300					
18	.761	.222	.168942	11	.730	.211	.154030	7	"	.168942	75	377
19	.761	.222	.168942	12	.720	.206	.148320					
20	.762	.221	.168402					8	"	.168181	75	377
21	.761	.222	.168942					9	"	.167497	75	377
22	.762	.222	.169164					10	"	.169164	75	377
23	.762	.221	.168402					11	"	.168942	67	374
24	.762	.220	.167640					12	"	.168402	67	374
25	.761	.221	.168181					13	"	.168046	67	374
26	.762	.221	.168402									
27	.762	.221	.168402					14		.168497	67	374
27½	.762	.221	.168402									
								15		.168737	67	378
Mean of 29			.168420						Mean of 15	.168686		
Maximum			.169608									
Minimum			.165895									
Mean of the 2			.167751									
Diff. of the 2			.003713									

TABLE LXXXVII.

ments. Drawn into two bars (A & B.) Reduced to a uniform size by filing, gauged at every inch from 0 *to* 27½, *inclusive. Specific gravity*, 7.7555.

Breaking weight × leverage.	Friction.	Effective strain.	Strength in lbs. per square inch.	Point of fracture.	REMARKS.
8820	441	8379	49597	No. 17	Part in tin from 4 to 8. Temperature rose once above 650°.
10500	525	9975	58924	0	The temperature rose to 660° when the bar appeared to be breaking. Having allowed the temperature to abate until it descended to 575°. Added weights until it appeared to be again breaking with 335 lbs. in the scale. Some tin then escaped at the packing, and 15 lbs. more were required to break it when cold.
10800	540	10260	60651	14	
11010	550	10460	61915	12½	
10440	522	9918	58895	27¼	Part in tin from 21 to 24½.
11310	565	10745	63518	7	Part under trial from 1 to 12.
11310	565	10745	63602	10⅓	Section of fracture gauged after this experiment, .586×.153 = .089658.
11310	565	10745	63889	6	
11310	565	10745	64150	4½	
11310	565	10745	63518	9¼	
11220	561	10659	63092	18½	
11220	561	10659	63295	26	
11220	561	10659	63429	24¾	
11220	561	10659	63259	22⅞	
11340	567	10773	63851	20⅝	
					The mean area of 15 sections of fracture, is *less* than that of 29 measured sections by .000266 square inch.

TABLE LXXXVIII.

Experiments on bar No. 222 B. Manufactured by the Salisbury Iron Company, at Salisbury, in Connecticut. The ore obtained from "Ore Hill," in that town. The metal was a mixture of "dead gray," "lively gray," "mottled," and "white" pig. Refined after the Eng-

Marks.	Breadth.	Thickness.	Areas of measured sections before trial.	No. of the experim't.	DATE.	Area of the section of fracture before trial.	Temperature, Fah.	Breaking weight in the scale.	Breaking weight × leverage.	Friction.
0	.752	.217	.163184		1833.					
1	.757	.217	.164269	1	Mar. 28,	.166100	560.°	298	8940	447
2	.757	.218	.165026							
3	.757	.219	.165783	2	"	.163455	576.	308	9240	462
4	.757	.219	.165783							
5	.756	.219	.165564	3	"	.166100	566.	320	9600	480
6	.755	.217	.163835							
7	.754	.216	.162864	4	"	.166320	554.	348	10440	522
8	.754	.217	.163618							
9	.755	.219	.165345	5	Mar. 30,	.163241	566.	349	10470	523
10	.755	.219	.165345							
11	.757	.220	.166540	6	"	.163998	66.	349	10470	523
12	.757	.220	.166540							
13	.756	.221	.167076	7	"	.162864	66.	374	11220	561
14	.756	.220	.166320							
15	.756	.220	.166320	8	"	.165026	65.	374	11220	561
16	.756	.220	.166320							
17	.755	.220	.166100	9	"	.164267	65.	375	11250	562
18	.755	.220	.166100							
19	.755	.220	.166100	10	"	.165783	65.	375	11250	562
20	.755	.220	.166100							
21	.754	.218	.164372	11	"	.166100	554.	332	9960	498
22	.754	.218	.164372							
23	.754	.218	.164372	12	"	.165816	578.	345	10350	517
24	.756	.219	.165564							
25	.756	.220	.166320	13	"	.167076	64.	369	11070	553
26	.756	.220	.166320							
27	.756	.220	.166320	14	"	.166540	64.5	370	11200	555
27.7	.756	.219	.165564							
				15	April. 4,	.165753	75.5	374	11220	561
Mean of 29			.165425							
				16	"	.165236	74.	369	11070	553
Maximum			.167076							
Minimum			.162864	17	"	.164372	74.	369	11070	553
Mean of these 2			.164970		Mean of 17	.165178				
Diff. of the 2			.004212							

TABLE LXXXVIII.

lish method. A loup formed and a gun bar drawn, expressly for these experiments. Reduced to a nearly uniform size by filing, and gauged at every inch from 0 *to* 27.7. *Specific gravity,* 7.7555.

Effective strain.	Strength in lbs. per square inch.	Point fractured.	REMARKS.
8493	51132	No. 18½	Part in tin from 5½ to 9.
8778	53703	" 0¼	Broke near the wedges.
9120	54907	" 17⅛	Broke near the wedges on the opposite side of tin bath.
9918	59632	" 15⅓	Do.
9947	60934	" 7½	Broke in the tin.
9947	60653	" 0¾	
10659	65447	" 7	Cut off the tinned part in the gripe of the wedges.
10659	64589	" 2	
10688	65064	" 5¾	
10688	64469	" 3⅞	
9462	56966	" 20	Part in tin from 22¼ to 25½.
9833	59301	" 24⅓	Broke *in tin.* Temp. had been as high as 614°.
10517	62947	" 13	This part not heated.
10545	63318	" 11	
10659	64389	" 24¼	Part now under trial from 20 to 24⅓.
10517	63649	" 20½	
10517	63983	" 21½	
			The mean area of the 17 sections of fracture is .000247 square inch *less* than that of the 29 measured sections.

TABLE LXXXIX.

Comparative view of the influence of high temperatures on the strength of iron, as exhibited by 73 experiments on 47 different specimens of that metal, at 46 different temperatures, from

No. of the comparison	Temperature observed at the moment of frac.	Mark of the bar on which the trial was made.	Strength at ordinary temperatures.	No. of experiments at ordinary temperatures.	Strength at the temperature observed.	No. of experiments at high temperatures.	Amount of variation from uniformity in the cold experiments.	Effects of the heat expressed in parts of the original strength.	REMARKS.
1	212°	137	56736	1	67939	1		+ .197	
2	214	133	53176	1	61161	1		+ .150	
3	394	58	68356	1	71896	1		+ .052	
4	394	148	65143	1	69752	1		+ .070	
5	394	23	62646	2	67765	1	.1041	+ .081	
6	394	125	57182	1	63322	1		+ .107	
7	394	61	55297	5	61917	1	.2026	+ .119	
8	396	75	60433	3	62415	1	.0444	+ .031	
9	440	224D.	49782	4	59085	1	.0908	+ .187	
10	520	224B.	54934	4	58451	1	.0992	+ .064	
11	550	199A.	76986	4	79846	2	.0936	+ .037	The standard for the original strength may possibly be a little too high.
12	550	221A.	60518	4	60322	1	.1680	— .004	
13	552	14	52542	1	55932	1		+ .064	
14	554	218A.	58124	4	60412	1	.0730	+ .039	
15	554	22	54372	4	61680	3	.1919	+ .134	
16	560	224E.	50528	7	58824	1	.0605	+ .158	
17	562	224C.	53385	5	59623	1	.1919	+ .104	
18	563	60	60907	4	72588	2	.0460	+ .191	
19	564	74	51030	5	58284	1	.0764	+ .142	
20	568	9	67211	2	76763	1	.0601	+ .042	Standard probably too high for the mean strength.
21	572	219B.	66724	2	66620	1	.0325	— .002	
22	572	49	59607	3	62278	1	.0878	+ .045	
23	572	222B.	56165	4	60117	2	.1550	+ .070	
24	573	10	64511	1	67503	3		+ .046	
25	574	231	76071	5	65387	1	.1373	+ .014	
26	575	220A.	54263	4	60988	1	.0280	+ .124	
27	575	62	58376	3	70081	3	.0262	+ .200	
28	575	207	51924	5	63825	3	.1225	+ .229	
29	576	221B.	59234	5	66065	1	.1190	+ .115	
30	576	223B.	43386	6	50068	1	.0760	+ .154	

TABLE LXXXIX.

212° to 1317° *Fah., compared with the strength of each bar when tried at ordinary temperatures, the whole number of experiments at the latter being* 163.

No. of the comparison.	Temperature observed at the moment of frac.	Mark of the bar on which the trial was made.	Strength at ordinary temperature.	No. of exp. at ord. temp.	Strength at the temperature observed.	No. of exp. at high temp.	Amount of variation from uniformity in the cold experiments.	Effects of the heat in parts of the original strength.	REMARKS.
31	°577	164	58769	5	66929	2	.1214	+.139	
32	578	224A.	52406	5	59197	1	.0565	+.129	
33	578	223A.	45757	5	53465	1	.0896	+.168	
34	580	86	62156	3	77163	2	.0986	+.052	
35	590	220B.	59459	5	62966	1	.0680	+.058	
36	598	90	50316	5	57310	2	.2401	+.138	
37	630	219A	59530	4	60010	1	.1440	+.008	This experiment was on a part probably defective.
38	636	16	53543	1	50039	1	.1563	—.067	
39	662	150	59307	5	58181	1	.0644	—.019	
40	722	152	57133	3	54441	1	.0507	—.047	
41	732	14	52542	1	53378	1	.1310	+.016	
22	734	150	59397	1	57903	1	.0644	—.026	
43	766	16	56891	1	54819	1	.1563	—.037	
44	770	149	56825	2	54781	1	.0234	—.036	
45	824 and 814	214	59219	1	55892	1	.0413	—.073	
46	825	149	56825	2	56644	1	.0234	--.029	
47	932	214	59219	1	45531	1	.0413	—.240	
78	947	232	58341	2	42401	1	.0446	—.273	
49	1022	214	59219	2	37410	1	.0413	—.369	
50	1037	152	58992	1	37764	1	.0507	—.360	
51	1097	227	53426	6	27604	1	.0330	—.483	
52	1111	227	53426	6	27602	1	.0330	—.483	The metal was decidedly defective at the point where this fracture was made—flaws visible.
53	1142	226	54758	2	18672	1	.1147	—.659	
54	1155	227	53426	6	21967	1	.0330	—.589	
55	1159	229	55774	3	25620	1	.1102	—.538	The 8th experiment on this bar being taken as the standard would exhibit the effect—.550.
56	1187	227	53426	6	21910	1	.0330	—.589	
57	1235	226	54758	2	21298	1	.1147	—.611	
58	1245	226	54758	2	20703	1	.1147	—.622	
59	1317	226	54758	2	18913	1	.1147	—.654	
		Mean	57525						

3*

Effect of high temperature on iron.

The experiments on bars of iron at high temperatures, were made either on sections deeply filed, or on those specimens which had been reduced by filing to a uniform size.

The trials below 600° were chiefly conducted in a bath of oil, arranged round the bar as already represented in Plates III. and IV., and the temperatures marked by the mercurial thermometer. For temperatures above that point the bath of tin and lead was substituted, and, when necessary, the steam pyrometer took the place of the common thermometer.

The view already presented of the influence of heat on copper, indicated partly by each of these two instruments, has enabled us to observe that they connect themselves in their indications in a manner to prove that no serious errors can be anticipated in the temperatures assigned in the higher parts of the scale when operating on iron.

If, however, in examining the effect of temperature on copper we meet with some difficulties in consequence of the irregularities of structure in the material, of want of conformity in different bars, and of the occasional weakening effects of alloying, on the total tenacity as we approach a red heat, the obstacles there encountered are comparatively trifling, when contrasted with those which are to be surmounted in the investigation of the effects of heat upon the tenacity of iron. Here we have, not only the variations due to the original composition of the metal; the differences resulting from the variety of pig-metal used in its manufacture, and the defects of the mechanical structure, owing to the want of uniformity in welding, or of regularity in the temperature of working the bars; but we have superadded to all these, a singular anomaly in the effect of heat itself on the tenacity of this material, which is believed never to have been before made the object of special inquiry.

Notwithstanding these impediments, the committee have not felt authorized to leave so important a point of inquiry, without a faithful attempt to unravel its intricacies. It would have been easy to devise a set of experiments, which, for a theoretical purpose, might have afforded to the analyst some interesting problems, and probably served to clear the subject of heat from certain difficulties with which its investigation is encumbered. Such, however, was not the purpose in view of the committee.

When we attempt to form a scale of the weakening effects of elevated temperatures, founded, as in the case of copper, on trials at ordinary temperatures, or even at the freezing point, we shall find that many of the first numbers in the scale will be negative, instead of positive, and this will continue to different points of temperature, according to the nature or condition of the iron on which the experiments are made. In fact, some of the very first experiments at high temperatures rendered this manifest, by showing that on a bar of uniform size, the fracture would not take place within the heating bath; and even that *much* filing of the part in the oil or melted metal, was necessary in order to prevent the fracture from taking place at unfiled sections *out of the hot bath* rather than at the filed one in it. This circumstance was noted at 212°, 392°, and 572°, rising by steps of 180° each from 32°, at which last point some trials had been made in melting ice. At the highest of these points, however, it was perceived that some specimens of the metal exhibited but little, if any, superiority of strength over that which they had possessed when cold, while others allowed of being heated nearly to the boiling point of mercury before they manifested any decided indication of a weakening effect from increase of temperature.

It hence became apparent that any law, taking for a basis the strength of iron in its ordinary condition, and at common temperatures, must be liable to great uncertainty, in regard to its application to different specimens of the metal. It was evident that the anomaly above referred to, must be only apparent, and that the tenacity actually exhibited at 572°, as well as that which prevails while the iron is in the state in which it was left by forging, or rolling, must be below its maximum tenacity. To determine what ratio exists between the ordinary strength of a bar and its maximum strength when in the most favourable condition for resisting a longitudinal strain, experiments were made on several bars by heating them to 572°, and then applying weight enough to cause a fracture, either within or without the heated part. The bar was then taken out and allowed to cool, when the strength which was obtained on parts influenced by the heat became a standard of comparison for experiments at more elevated temperatures. A mean of thirty-five comparisons, conducted in the manner just described, afforded a standard 16.2 per cent. greater than the ordinary strength of the metal; but the standard most relied on for furnishing the basis of calculations, and for determining a law of diminution of tenacity, was derived from the five varieties of iron, manufactured by the Salisbury Iron Company, which, being of a tolerably uniform texture, were considered rather more suitable than others for supplying the ground work of a law for calculating the effect of temperature on this metal generally. An examination of the trials on those bars will be found to furnish a standard of maximum tenacity 15.17 per cent. greater than their mean strength when tried cold. When, however, an unexceptionable standard was given by any bar after trial at 572° and subsequent cooling off, its own standard for increased strength was used in computing the true effect of heat at other high temperatures.

Thus, at a temperature of 1317°, the bar No. 226, which had possessed, when cold, a strength of 54758 lbs., gave a remaining strength of only 18913 lbs. Now, 54758 lbs. increased 15.17 of itself, gives 63065, and from this deducting 18913 we have 44152 lbs. for the *diminution* of its absolute tenacity by the temperature just mentioned, or .7001 of the maximum strength.

On the same bar, (No. 226,) were made at different points, two other experiments with the same weight each time in the scale.

The first of these sections gave way when the temperature had reached 1237°. The strength per square inch given in this case was 21298, and comparing this with the maximum strength, 63065, we obtain 41767 as the diminution, equal to .6622 of that maximum.

The second trial on a larger area of section required a higher temperature to cause the fracture to take place under the given weight, viz: 1245°, giving at this temperature a tenacity of 20703 lbs., and by the same computation showing a diminution from the maximum 63065 of .6715. Both of these trials having been made with the precaution of raising and lowering the suspended furnace, to regulate the heat, it is believed that no essential error in regard to temperature can have existed. The first was conjectured to be, if anything, a trifle in excess.

If we take the mean of these two results, viz. .6668 for the diminution of tenacity at 1241° the mean temperature, it cannot vary far from the true effect. On bar 227 an experiment was made at 1187°, giving a tenacity of 21913 lbs. per square inch. Within two and a half inches of the same point a cold fracture gave a strength of 52186 lbs., from which the calculated maximum is 60102, and the diminution is 60102—21913, or 38189; which is .6352 of the same maximum tenacity.

On No. 229 was made an experiment at 1159°, which exhibited a tenacity of 25620 lbs. Three experiments on the same bar when cold, gave a mean strength of 55774 lbs. Hence $55774 \times .1517 = 8460$; and $(55774 + 8460) - 25620 = 38614$, which is .6011 of the maximum tenacity.

On No. 227 we have an experiment at 1155°, giving a tenacity of 21967, and the four cold experiments nearest to the same point give a mean of 47749, from which we obtain the maximum 54992, and the diminution = .6000.

On No. 226 was made an experiment at 1142°, but as the iron at the part in which the fracture took place was defective from flaws, and had probably been impaired by the previous straining of the bar, it was not considered necessary to attempt to reduce its apparent tenacity to the standard, being entirely anomalous.

At 1111° the bar No. 227 had a strength of 27602, and another trial on the same at 1097°, 27602.

The weight in the scale was the same in both cases, and the temperatures would probably have been the same, had not the standard piece in the latter case accidentally risen above the melted lead a short distance just before the fracture. Taking the mean of these two results 27603, for the strength at 1111°, and the mean of six trials on this bar near the two points where these fractures occurred, viz: 53426, we obtain the maximum tenacity at those points 61531, and the diminution by heat .5614.

On bar 152 an experiment at 1037° gave a tenacity of 37764, and on No. 214 an experiment at 1022° gave 37410. The mean cold strength of these two bars was 59105, from which we deduce the maximum 68071; and the diminution for the mean temperature 1030°, equal to .4478 of the maximum.

At 947° an experiment on bar No. 232 gave a strength of 42401, the mean of the two experiments subsequently made nearest to this point gives the experimental maximum strength 66193 from which the diminution is .3593.

At 932° bar No. 214 had a tenacity of 45531, while its cold strength was 59319, and its maximum 68202, hence the diminution is .3324.

An experiment was made on bar 149 at a temperature marked 825°, but as the furnace was not lowered during the performance of it, and as the time during which the bar continued to stretch after the strength had been fairly overcome, was considerable, the temperature is in all probability too high; and the experiment is not considered comparable with the rest of the series.

In bar No. 214, at the temperature of 824°, the remaining strength was 55892, the original strength, 60850, and the maximum by calculation, 70080, whence the diminution is .2010.

On bar 149 was an experiment at 770°, giving a tenacity of 54781; while the original strength was 56825, and the diminution from the calculated maximum .1627.

On No. 16 we find an experiment at 766°, giving 54819. Two subsequent experiments yielded maxima, the mean of which is 65176, whence the diminution is .1586.

In bar No. 150, a temperature of 734°, left a strength of 57903. The first experiment on the bar afforded 59397, from which we calculate the maximum 68407, which proves the diminution at this temperature to be .1535.

On No. 14 we obtained a strength of 53378, at 732°, and the mean of three experimental maxima, is 62736, hence the diminution by heat is .1491.

On No. 152 we had at 722° a tenacity of 54442, and three experiments gave a *cold strength* of 55990, from which a calculated maximum of 64483 is obtained, and consequently a diminution of .1557. But an experimental maximum of 62709 was obtained on this bar, which on account of the remoteness of the point where it occurred, from the point on which the hot fracture was made, is believed to be rather too low. Calculating, however, from this maximum, we find the diminution .1316.

If we take the mean of the two results, .1557 and .1316, we have the probable diminution from the true maximum, .1436.

On No. 150 we find an experiment at 662°, giving a tenacity of 58182. On the same bar an experimental maximum was found of 65785, from which we get the diminution equal to .1155.

On No. 16 was made a trial at 636°, yielding a result of 50039, a result far lower than that given afterwards on the same bar at 766°; we are therefore compelled to believe that this experiment was made on a defective part of the bar.

On No. 219 A, was a trial at 630°, which exhibited a tenacity of 60010. An experiment subsequently made within 1¾ inches of the same point, gave a tenacity of 67033, and consequently the diminution is .1047.

On No. 90 an experiment at 600° gave a tenacity of 56938, and three experiments on the same bar, when cold, gave a mean of 54715, from which the calculated maximum is 63015, and the diminution .0964.

On the same bar (No. 90,) another trial took place at 596°, giving a strength of 57682 lbs., and if we assume the original strength of this section equal to that given by the third experiment on the same bar, 55037, we shall have the maximum by calculation 63386, and the diminution .0899.

By a mean of 5 sets of experimental maxima derived from 65 trials on the 5 varieties of Salisbury iron we have a standard of 66146. The six trials at the mean temperature of 570° referred to in our remarks in Table LXXIX. of the effect of employing different kinds of pig-metal, show that at a mean temperature of 570° those trials gave a strength of 60398 lbs., whence the diminution is .0869.

Of 224 B, at 520° the tenacity was 58451. On the same bar, four cold experiments gave a mean strength of 54934, which by calculation gives a maximum of 63267 and a consequent diminution of .0761.

On a survey of the preceding discussions it will be seen that in determining the maximum belonging to each point of fracture, it has been necessary to resort sometimes to experimental, and sometimes to calculated results, but that in several cases the two operate as checks upon each other.

On attempting to extend the principle to trials made below the temperatures already cited, we are liable to encounter an ambiguity in the results, owing to the fact that the maximum tenacity is not generally to be obtained without having carried the previous temperatures to about 550° or 600°, and the tension to nearly or quite that of the original strength of the metal when cold.

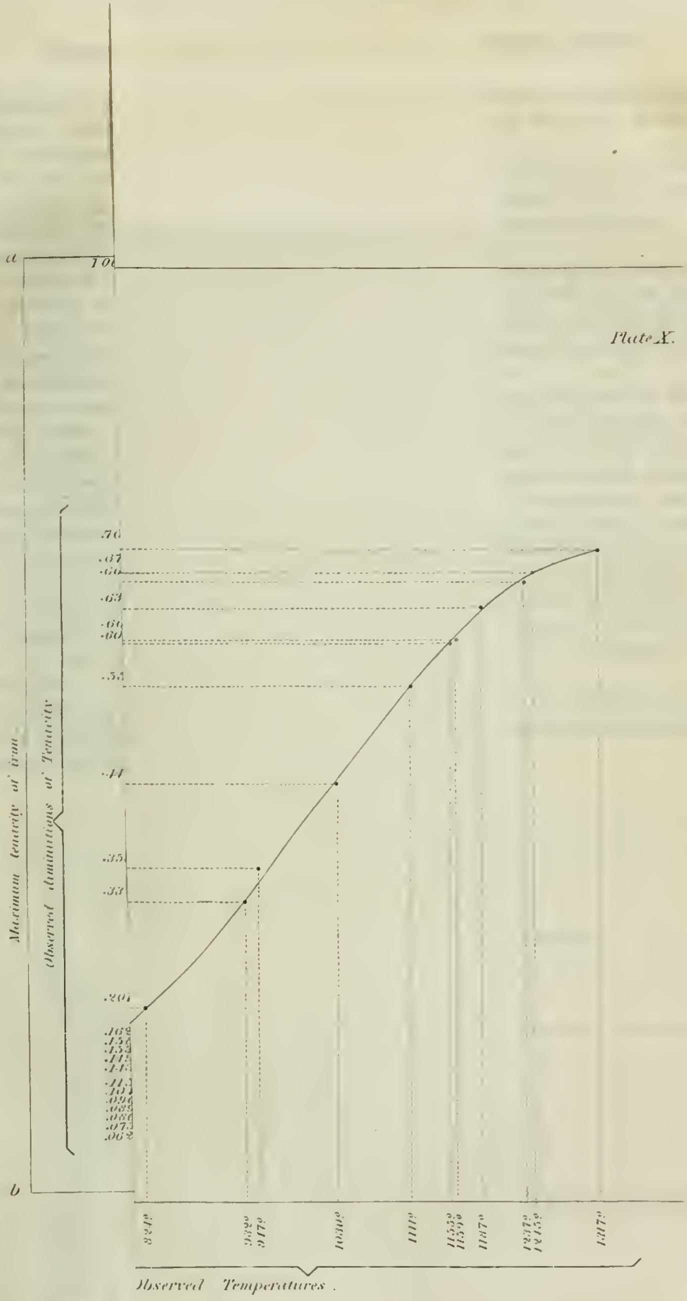
In projecting into a curve as in Plate X. the data furnished by the experiments above described, and of which a synopsis is given in the following table, it becomes at once apparent that what was *conjectured* with respect to copper, in regard to a point of inflection, is here presented in a manner to admit of no uncertainty. Indeed it could hardly be otherwise, when we consider that the melting point of wrought iron, at which all tenacity must be overcome, is doubtless situated above 3000° ; and by the experiments of Clement and Desormes, is as high as 3945°. Now it appears that at a temperature no higher than about 1050° one-half of the strength is destroyed ; at 1240°, two-thirds ; and at 1317°, seven-tenths of the maximum tenacity is overcome.

The following table exhibits the observed temperatures, and corresponding tenacity of the metal with the calculated, or experimental maximum of strength,—the ratio of the observed diminution to the maximum tenacity, and the irregularity of the metal in parts of the original strength at ordinary temperatures.

TABLE XC.

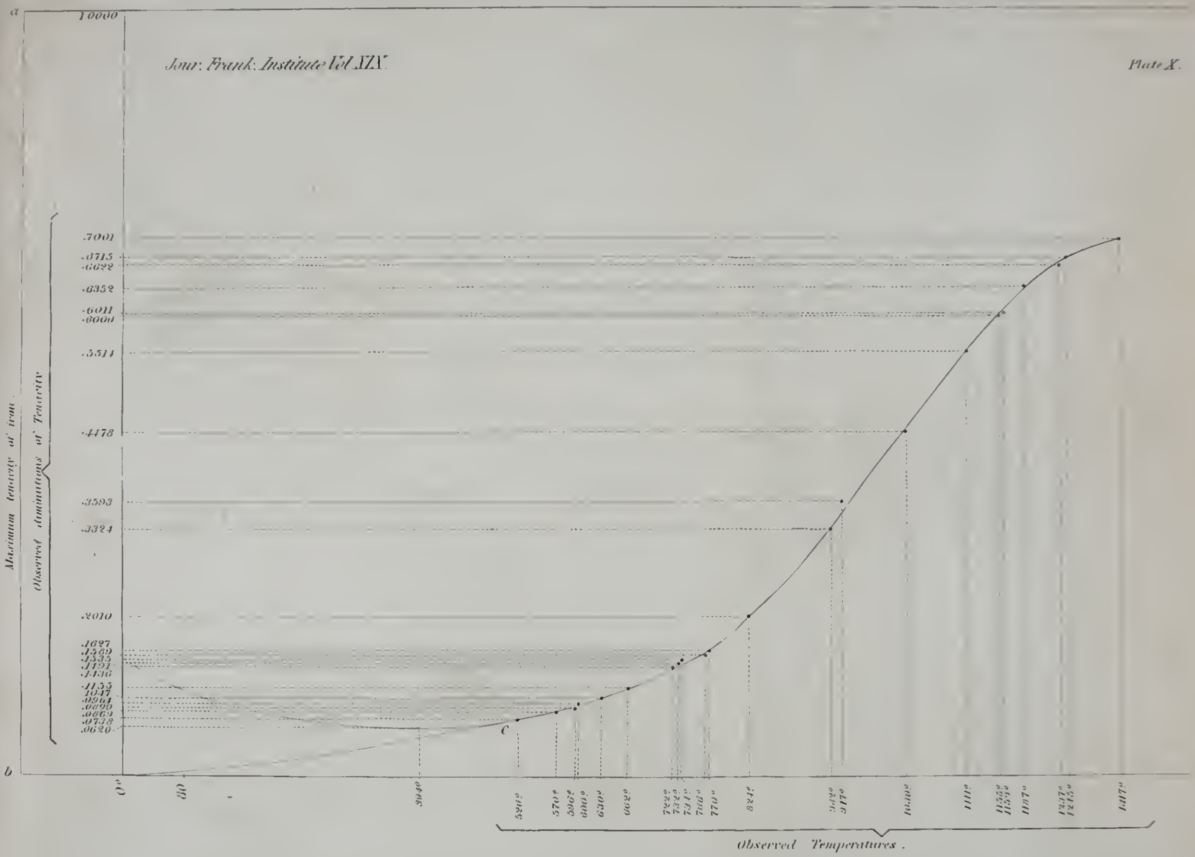
No. of the comparison.	Marks of the bar.	Temperature observed.	Tenacity observed.	Maximum tenacity at the point of fracture.	Manner in which the maximum was obtained.	Diminution by heat in parts of the maximum tenacity.	Irregularity of the metal in parts of the original strength.
1	224 B.	520°	58451	63275	Experiment.	.0738	.0992
2	Salisb. iron.	570	60398	60398	do.	.0869	.1125
3	90	596	57682	57682	Calculation.	.0899	.2401
4	90	600	56938	63086	do.	.0964	.2401
5	219 A.	630	60010	67033	Experiment.	.1047	.1440
6	150	662	58182	65785	do.	.1155	.0644
7	152	722	54442	64483	Calculation.	.1436	.0507
8	14	732	53378	62736	Experiment.	.1491	.1310
9	150	734	57903	68407	Calculation.	.1535	.0644
10	16	766	54819	65176	Experiment.	.1589	.1563
11	149	770	54781	65445	Calculation.	.1627	.0234
12	214	824	55892	70080	do.	.2010	.0413
12	214	932	45531	68202	do.	.3324	.0413
14	232	947	42401	66193	Experiment.	.3593	.0446
15	{ 214 } { 152 }	1030	37587	68071	Calculation.	.4478	.0460
16	227	1111	27603	61531	do.	.5514	.0330
17	227	1155	21967	54992	do.	.6000	.0330
18	229	1159	25620	64234	do.	.6011	.1102
19	227	1187	21913	60102	do.	.6352	.0330
20	226	1237	21298	63065	do.	.6622	.1147
21	226	1245	20703	63065	do.	.6715	.1147
22	226	1317	18913	63065	do.	.7001	.1147

Plate X.


a
100
b
Maximum tenacity of iron
Observed diminutions of Tenacity
.70
.67
.66
.63
.66
.60
.54
.44
.35
.33
.201
.162
.156
.153
.115
.113
.113
.101
.094
.089
.086
.073
.062
824°
932°
917°
1030°
1111°
1153°
1159°
1187°
1237°
1245°
1317°
Observed Temperatures.

Jour: Frank: Institute Vol XIX.

Plate X.



From the eighth column of the preceding table it appears that of these 15 different specimens of iron, the mean irregularity of structure is 10 per cent. of the mean strength when tried cold.

For the purpose of ascertaining, approximately, the law of decrease in strength by temperature, an investigation was made similar to that adopted for copper, embracing, however, only 12 of the points contained in the preceding table.

As some of the experiments which furnished the standards of comparison for strength at ordinary temperatures, were made at 80°, and as at that point small variations in respect to heat appear to affect but very slightly the tenacity of iron, it was conceived that for practical purposes at least, the calculations might be commenced from that point.

Eighty degrees are therefore deducted from each temperature in the following table, and the remainders used, instead of the numbers commencing from the 0 of our scale. It will be found that with the exception of a slight anomaly between 520° and 570°, amounting to —.08, the numbers expressing the ratio between the elevations of temperature, and the diminutions of tenacity, constantly increase until we reach 932°, at which it is 2.97, and that from this point the ratio of diminution decreases to the limits of our range of trials, 1317°, where it is 2.14. It will also be observed, that the diminution of tenacity at 932°, where the law changes from an increasing to a decreasing rate of diminution, is almost precisely one-third of the total, or *maximum* strength, of the iron at ordinary temperatures.

At this point it will be seen, the curve traced in the figure, Plate X., undergoes an inflection, and in all probability continues in the same general direction to the fusing point.

TABLE XCI.

No. of the comparison.	Observed temperatures.	Observed temperatures —80°.	Observed diminution of tenacity.	Power of the temperature which represents the diminution of tenacity at each point.	REMARKS.
1	520°	440°	.0738	2.25	
2	570	490	.0869	2.17	
3	596	516	.0899	2.38	
4	662	582	.1155	2.67	
5	770	690	.1627	2.85	
6	824	744	.2010	2.94	
7	932	852	.3324	2.97	Point of inflection near this temperature.
8	1030	950	.4478	2.92	
9	1111	1031	.5514	2.63	
10	1155	1075	.6000	2.60	
11	1237	1157	.6622	2.41	
12	1317	1237	.7001	2.14	
			Mean	2.58	

From the above table it appears that the ratio of diminution furnished by a comparison of some of the lower temperatures with all those above them is higher than the duplicate. The same inference is derived from a comparison of the higher members of the series with all those below them.

At 932° it will be seen that a comparison with all those both above and below that temperature, gives a rate very nearly approaching to the cube. The particular comparison between 824° and 932° gives a rate higher than the 4th power, viz. 4.08.

Hence though the diversity of the metals operated on, is such as not readily to furnish the precise mathematical law, it is still abundantly apparent that this law must be different from that which is indicated by any one of the family of parabolas.*

But for practical purposes the table indicates, by the *mean* of all the rates, that a rule may be followed, not widely different from what is represented by saying that the *thirteenth power of the temperature above* 80° *is proportionate to the* 5*th power of the diminution from the maximum tenacity*.†

Plate X. exhibits at .1517, in the line of observed diminution, the commencement of a branch of the curve descending to the right, which indicates the progressive effects of temperature, increasing, as it rises, the tenacity of iron until a certain point is reached, when the weakening influence begins to be felt. The other branch of the curve, or that which takes its rise from the origin of the abscissas, forms, with the first, a cusp of peculiar character at a point *c*, which, however, the experiments are not sufficiently numerous in this part of the scale to determine exactly in respect to *position*.

From the preceding discussion Table LXXXIX. will be sufficiently intelligible without further comment.

Elasticity of Iron.

In describing the method of determining the elasticity of the *machine* for tenacities, we have given in effect a detail of the processes also pursued with the bars of iron generally.

The strong bar then interposed between the heads, *b*′, *b*″, (Plate I.) was, however, now replaced by the specimen under trial, and as the machine was capable of overcoming the total strength of each specimen, the temporary elongations, as well before as after the bar had begun to be permanently extended, were easily deducible from the observed elasticity of the *bar and of the machine together*, corrected by deducting the already ascertained elasticity of the machine alone.

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* Represented by the well known general formula, $y^q = mx^p$.

† It is evident from what has already been said, that if we would *calculate* the reduction from the ordinary strength of iron as it comes from the hammer or the rolls, we must first reduce it to the maximum, by adding to its observed strength at a known low temperature, 15.17 per cent. of itself. It is, moreover, apparent from the curve, as well as from the table, that if we conceive the ordinates (y) or increments of temperature, and the abscissas (x) or decrements of tenacity to be drawn from any two points *below* 932°, the increase (dx) of any abscissa, for the contemporaneous increase (dy) of the corresponding ordinate, will observe an *increasing rate;* but *above* that point where also the *differential co-efficient* $\frac{dy}{dx}$, is a maximum, and where the *differential co-efficient of the second order* $\frac{d^2y}{dx^2} = O$, they observe a decreasing rate and the concavity of the curve is accordingly directed towards the axis of the abscissas, in place of the convexity which had hitherto inclined in that direction.

By referring to a former part of this report (Table III.) there will be found a series of numbers corresponding to the several observed elasticities of the machine, used by the committee, and representing in inches the actual distances, or quantities of recoil after the strain had been removed. From those numbers may be taken out *by inspection*, the quantity of recoil for each trial in the following table, and comparing the results thus obtained with the total lengths of the bars, it will be seen by what part of its whole length, each was elongated and contracted at every trial.

It will not fail to be remarked that in the more extended series of the table, those for example, in which 7 or 8 trials were made on the same bar, the maximum of elasticity was often found within a comparatively small number of pounds of the *breaking weight*, and that it was seldom so low as two-thirds of that weight. This is at variance with the supposition that the elasticity of a bar is destroyed or much diminished at the moment it has begun to be permanently elongated.

(TO BE CONTINUED.)

FOR THE JOURNAL OF THE FRANKLIN INSTITUTE.

Observations on the duty performed by the Cornwall Steam Engines. By JACOB PERKINS, Civil Engineer.

Read before the Institution of Civil Engineers.

No. III.

When the paper on the expansive property of highly elastic steam, was read on the 8th inst., and accompanied by a sectional drawing of a double cylinder single-stroke engine of my invention, it was said by one of the members of the Institution, that it was Woolf's engine which I had produced as being my own. Now, sir, it so happens that the two engines in question are entirely dissimilar in principle and effect. It is difficult to believe that the gentleman who made this declaration took the trouble to examine the drawing before he accused me of claiming another person's invention; had he done so, one of his penetration would, at a glance certainly, have perceived an obvious difference. Mr. Woolf's engine was one of great promise on account of its ingenious arrangement. It was supposed that it would embrace the whole advantage of the expansive property of high steam. It however was found that if the same steam had been admitted directly into the large cylinder instead of first passing through the smaller cylinder, that more duty would have actually been performed. This unlooked for fact confounded the supporters of this engine and puzzled every one. To trace out the cause of this unexpected result we have only to imagine the small cylinder to be filled with steam of, say 40 lb. per inch, the steam also to be let into the large cylinder at the same pressure, then the small piston would be neutralized and would have no tendency to move in any direction, but since the steam could find its way between the large piston and the head of the large cylinder, both pistons will begin to move in the same direction. It will be seen that the steam which is acting on the large piston is reacting against the small one, and were both the pistons of a size no movement would take place.

The most important distinction between Mr. Woolf's engine and mine is that in his engine the steam is always pressing on one side or the other of the large piston, and always pressing on *both* sides of the small one, thereby creating great reaction on the small piston. Each end of his large cylinder is alternately open to the condenser, the small one never.

In my arrangement it will be seen that the steam is never on both pistons at the *same time*, for one piston or the other is always working *without* steam pressing on it. Another trait which belongs to no other engine within my knowledge is, that of the large cylinder being *always* open at one end to the condenser, and the small one half the time to it, producing the important result of removing *all* cause of reaction. I think it cannot but be acknowledged that there is difference enough in action and effect to constitute mine an entirely different engine.

Mr. J. Hornblower was undoubtedly the first who used the double cylinder expansive engine. This engine failed, however, and Mr. Woolf comes next with his double cylinder expansive engine. Mr. W.'s engine although upon the same general principle as Mr. H.'s, possessed much more power, and was of course much improved. This was unquestionably owing to Mr. Woolf's using much higher steam, and his having constructed a very ingenious boiler, which generated much more steam (if attended by a skilful stoker,) than any other kind of boiler then extant. It was found, however, that in practice, too much attention and skill was necessary to make it profitable, and it was given up. A few days' work was enough to destroy a new boiler if not properly attended to; the repellant property of heat was more perceptible in this boiler than in any other. Mr. Woolf, in answer to some questions put to him, admitted that, at times, the water entirely left the small tubes and rested in the steam chamber, making at the same time very little steam, and his small tubes would then either crack or melt. This, he observed, was fatal to his experiment, which at first promised so much.

According to the Cornwall reports, Mr. Woolf's engine, at one time, raised 57,000,000 lbs. one foot high with a bushel of coals, while at the same time the utmost duty done by Mr. Watt's single-stroke, balance-bob, expansive engine was 33,000,000 lbs.

If Mr. Woolf had dispensed with the little intermediate cylinder altogether, his engine would have done much more duty; for his large cylinder worked with 40 lbs. steam expansively, and his boiler generated much more steam at times than the present Cornish boiler. Mr. Woolf soon abandoned his smaller cylinder, which showed that the practical trial had taught him its inutility.

There is no gain that can mathematically be estimated in the use of two cylinders, and as long as the pressure of the steam falls short of about 200 lbs. per inch, a single cylinder will operate much better, and will produce all the advantages to be obtained by the expansive property of high steam. The only use of the small cylinder is that of taking off the strain upon the engine when the pressure exceeds a certain point. It will be seen that if the steam had been let on to the large piston at 600 lbs. the engine must have been inconveniently strong to have borne the strain. It would have been impracticable in such a cylinder, to have cut off the steam at $\frac{1}{36}$ part of the stroke, but by the use of the small cylinder, I was enabled to allow the engine to make one-fourth of the stroke before the steam was stopped off, giving time enough to remove all practical difficulties.

You say, observed one engineer, that Woolf lost much power by using two cylinders, and yet you try to prove that with a little alteration, or modification, there is a great advantage in using two; I should like to know how this is? I still maintain that one cylinder is far better than two where steam as low as 40 lbs. per inch is used. I by no means would recommend two cylinders, unless the steam to be used is considerably above 200 lbs.

It will be seen that Woolf did not use his intermediate cylinder to soften down the stroke, for the steam was admitted on to the large piston at the same pressure that it entered the small one.

I have thus endeavoured to point out the causes of the failure of these two very ingenious engineers in the novel attempt of applying the valuable power arising from the expansive power of steam through the agency of two cylinders, but I hope that it will not be believed that I have done so from any unworthy motive. Greater credit is due to the originator of a new principle, although he fail in realizing his expectations, than to the more fortunate individual who through his errors sees the cause of his failure, and benefits by his want of success. It is by observing our own errors, and those of others, that we are enabled to arrive at truth.

JACOB PERKINS.

FOR THE JOURNAL OF THE FRANKLIN INSTITUTE.

Observations on the circumstances attending the Explosion, and Bursting of Steam Boilers; with animadversions on the Experiments of the Franklin Institute, on that subject. By JACOB PERKINS, Civil Engineer.

A series of experiments to discover the cause of the explosions of steam boilers, was ordered to be made, by the Secretary of the Treasury, of the United States of America, and the Franklin Institute was selected to make the experiments; and certainly no pains have been spared by that Institution in its researches; but from the want of some practical facts which have transpired in the course of the experiments which I have made within the last twelve years, they have, as I believe, arrived at some conclusions entirely fallacious, and which will, if not controverted, tend to injurious consequences. The most dangerous of these conclusions is, that all the destructive explosions of steam boilers, have been caused by the direct pressure of the steam, and that there is no difference between the explosion, and the bursting, of a steam boiler. If the late Oliver Evans, (who in his own department was one of the greatest men America has ever produced) had been still living he would have given a very different opinion. Mr. Evans had a record of more than 600 burstings, before he had one explosion, although the pressure was greater when the bursting took place, than when the explosion happened. I have myself witnessed enough to be perfectly satisfied that there is as much difference between a bursting and an explosion of a steam boiler, as there is between the bursting of a cannon by hydraulic pressure, or by gun-powder. It is well known that a cannon bursted by hydraulic pressure is perfectly harmless, and the effect of bursting with gun-powder is also known by dreadful experience. In my endeavours to learn the nature and value of high steam, I have been so fortunate as never to have had one explosion, although I have very often had the steam up to a pressure of 100 atmospheres; but I have had more than 100 rends, or burstings. In the Adelaide Gallery where the steam has been up daily to an average of 450 lbs. to the square inch, for more than four years, and where the exhibition of the steam gun has been many times stopped in consequence of the bursting of the generator, yet no report has ever been heard, even by persons within a few feet of the generator. In fact I never knew a brick to be disturbed by the bursting of the generator. About ten years since I was exhibiting the steam gun in a large assemblage, when in the middle of a volley the steam dropped, and the balls refused to leave the barrel. I immediately ran to the furnace and found a rent about eight inches long and one inch wide in the centre of the boiler, which was three and a half feet long, seven inches diameter, and three-quarters of an inch thick, of wrought copper. I apologised to the company for stopping the experiment as the boiler had burst, they exclaimed that it was impossible, for they had heard no explosion, my answer was, come and see, which many did, when the astonishment manifested, as well as the pleasure expressed, by them, was very great. One gentleman in particular was extremely gratified at being present when the circumstance occurred, he being an owner of steam engines.

The Committee of the Franklin Institute have made experiments upon a subject much pursued by me, namely, to ascertain whether surcharged steam could have its power increased by adding sufficient water to give to it its proper density. In a pamphlet published by me about eight years since, containing observations upon the causes of the explosion of steam boilers, I offered some opinion on this point, which found many believers; the opinions then given were the result of much observation, and of numerous experiments; my experiments, however, to ascertain this fact, were conducted in a very different manner from those of the committee; I built my fire under the bottom of a very strong generator, which would bear a pressure of at least 300 atmospheres. The object of this great strength of the generator was not only to enable me to make my experiment without danger, but to surcharge the steam at a very high temperature, so that there should be no mistake as to the result. The fire was made at the bottom of the generator, about a quarter up the sides, and about as much above the water as below it, observing at the same time to have the fire much more intense above than below the water; but no fire was made *on*, or *near*, the top of the boiler; believing that heat would not descend any more in steam than in water. This fact I have now completely established by a new modification of a boiler which owes much of its success to the property which surcharged steam possesses of transmitting heat; the particulars of this modification cannot be made public until I have obtained my foreign patents therefor. If I had made the fire on the top of the boiler, as the Committee of the Franklin Institute did, in their experiments, I should have made the same mistake, and instead of surcharging the mass of steam, I should only have surcharged a small film next the heated metal, and have left the rest perfectly saturated with water, and quite unfit for receiving a jet of that fluid, which could only serve to lower the temperature and power, which was the case in their experiments.

At the top of my generator the temperature was at least 3000 degrees, as ascertained by an alloy that fused at about that point; the temperature of the steam in contact with the water was about 300 degrees, the mean temperature about 1500 degrees, that together with the heated sides and top of the generator furnished heat enough to make the indicator rise at each stroke of the pump from 50 to 100 atmospheres; and as the steam was constantly blowing off at the safety valve, which was loaded at five atmospheres, the steam would fall to this pressure in about fifteen seconds. The oscillation of the indicator was from four to five times per minute, its rise was instantaneous, but its fall gradual.

The 9th experiment of the committee was "to repeat Perkins' experiments and ascertain whether the repulsion, stated by him to exist between the particles of intensely heated iron and water, be general, and to measure, if possible, the extent of this repulsion with a view to determine the influence which it may have on safety valves." The experiment made with the perforated thick iron boilers, as far as it went, was very satisfactory. The water standing in a bowl heated up to 800° without passing through the metal, until the metal had lowered its temperature, would be easily seen by any one, and would be an interesting experiment to be repeated in a lecture room.

The committee now proceed to give an account of an unsuccessful attempt to repeat Perkins' experiment referred to in the query. As it does not seem to bear upon the application of the safety valve, they did not deem it necessary to encounter the expense of the apparatus necessary to a further trial. The experiment of Perkins which is more particularly referred to in the query, is that in which an opening having been made in one of his generators, containing intensely heated water in contact with the red hot metal, neither steam nor water escaped, and in which having affixed a pipe and stop-cock to the same vessel, no steam issued through the cock when opened. To repeat this with a view to ascertain, as required in the query, the size of the opening to which such a result would apply, three apertures were made of $\frac{1}{16}$th, $\frac{1}{8}$th and $\frac{1}{4}$th of an inch respectively, in the sides of a wrought iron mercury bottle; these were closed by conical plugs connected with levers, by which the plugs could be withdrawn from the sides of the bottle. The fulcrums of these levers were attached to the wrought iron cylinder already referred to, within which, its axis coinciding with that of the cylinder, the cylindric bottle was placed. An earthen-ware furnace was placed below the bottle and surrounding cylinder, the latter resting upon wrought iron bars supported by the edges of the furnace, and the former supported by a stone placed upon the grate of the furnace. Besides affording a support for the levers, the wrought iron cylinder was introduced to protect the experimenters against injury should the bottle explode in the trials to be made with it. The apparatus having been placed in a quarry pit, adjacent to that in which the cylindric boilers were burst, water was poured into the bottle so as to fill it; the screw plug was next passed into the neck and forced home by lateral blows from a hammer; a fire was now made in the furnace, and the fuel heaped up so as to fill the entire space between the mercury bottle and wrought iron cylinder, and to be about five inches deep above the stopper of the former; a string was attached to the lever connected with the smallest plug and carried up the bank. The fire soon burned briskly, and it was perceived that a small quantity of steam mingled with the feeble smoke and heated air which rose above the apparatus. About twenty minutes after the beginning of the experiments the leak appearing to increase, an incautious attempt was made to stop it, but without success; at this time the bottle was seen to be at a dull red heat. It was thought that but little water had been able to escape in steam through the very minute opening which the imperfect thread of the screw gave, and it was intended to withdraw one of the plugs when a few minutes should have elapsed to give time for the bottle to be heated to complete redness; meanwhile a most violent explosion occurred, the body of the bottle rose in the air, the iron cylinder which served to increase the height of the furnace was throw from its place, the earthen furnace blown to pieces, and the fire scattered far and wide through the woods. After extinguishing the fire, it was found that the iron cylinder, weighing, with the apparatus connected with it, 51$\frac{3}{4}$ lbs. had been thrown four feet from its bed, the plugs which passed into the bottle had been broken short off at the exterior of the bottle, the bottom had been forced into the ground, which was ploughed up by the fragments of the furnace, and completely wet for a considerable distance around: one of the iron bars supporting the cylinder was thrown a distance of thirty feet and sunk three inches into the ground. The body of the bottle was found thirty yards from its position before the explosion, having penetrated two feet into the ground. The noise of the explosion resembled that of a 12 pounder fully charged. This experiment proved first, that steam, from intensely heated water was able to penetrate an exceedingly small opening; although it proved nothing in regard to an aperture made in a vessel containing water only, it showed an effect produced when there was very little steam in the vessel. It verified the deduction from theory, that but a small part of water highly heated can expand into steam if suddenly relieved from pressure. "It showed that great danger must be incurred in attempts to heat water very highly even in vessels where it has but little room to expand itself, contrary to the opinions entertained by many, and that an attempt to repeat the experiment of Perkins, unless with an apparatus capable of sustaining the most intense pressure, must be attended with great danger." Although it appears that the boiler was first filled with waste water yet under such tremendous pressure the water and steam would be rapidly discharged through the small fissure which was observed to have made its appearance, and as the water lessened its bulk, so as to leave a space between the metal and water, it would be repelled from every part of the boiler except at the fissure, which by its blackness would have shown that the temperature of that spot was very much reduced by the issue of steam and water. If the whole boiler showed a dull redness, as was stated to have been the case by the committee, water could not have been in contact with the boiler, but must have been separated by a thin film of non-conducting material; for had the water been heated to dull redness, a boiler of twenty times the strength of that used, could not have resisted the resulting pressure. I have no doubt that the water was of a much lower temperature than the steam in contact with it, or the metal which was in contact with the steam. It was observed by the committee that just before the explosion the rush of steam increased. This was undoubtedly the cause of the explosion. It has been frequently observed that when the water had been reduced in the boiler so as to allow the heat to enter above the water and surcharge the steam, as well as heating the upper part of the boiler, that if at this time the safety valve be suddenly raised so much so as to take off the steam faster than it is generated, then the pressure would be taken off from the surface of the water so as to allow it to rise up in mist, and take off the extra heat from the surcharged steam, as well as the heat from the top of the boiler, and become completely saturated; at this time an explosion must necessarily take place. Now I conceive that the experimental boiler was in this predicament, and when the fissure had increased sufficiently to take off the pressure from the *cylinder of water* which was in the interior of the boiler, this water would burst into mist and take up the heat in the surcharged steam as well as the heat of the metal, and produce what was witnessed by the experimentalists. Now if the plug had been withdrawn before the explosion took place, steam and water would have rushed from the hole. How could it have been otherwise? was not every part of the heated boiler rendered repellent; the aperture was the only place for its escape; thus it would have been made to appear that the experiment that I published sometime since, was entirely fallacious. Now if the Committee will take the trouble to make the experiment as I made it, or as one was made by Alexander Gorden, Esq., of London, Civil Engineer, they will witness the fact. Mr. Gordon called upon me sometime since and informed me that he had made an experiment to prove whether the facts, that I had stated respecting the repellent power of heat, were correct. He observed the reason of his not having tried the experiment long before was, because he conceived it entirely contrary to the laws of nature, nor should he have tried it then had he not been called upon by Mr. Ogle, who had been much troubled with the burning out of the bottoms of his tubular boilers. Mr. Gordon stated to Mr. Ogle that if the facts, which I had published sometime since, were correct, it would be easily accounted for, and recommended an experiment, which was determined on. Mr. Ogle's boiler consisted of a group of tubes 3½ feet long,4 inches diameter, and under one-eighth of an inch thick, standing vertically, arranged very close, and the heat of the fire passing up between them. In one of these tubes a series of cocks which ranged from the bottom to the top, say four or five inches apart, were fixed for observing the changes of the water and steam at different temperatures, these cocks were about a quarter of an inch of interior space. The fire was now gently made under the bottom of the boiler, the steam was soon got up, and was allowed to blow off constantly, as fast as it was generated; these tubes contained about two-thirds water and the other third steam; when the lower cock was first opened water issued as was expected at the temperature; this cock was now closed and the top opened, when the steam rushed out at about 200 lbs. pressure to the inch; the bottom of the tube was now raised to a higher temperature and when the lower cock was opened, steam and water escaped; this cock was then closed and the upper one was opened, and steam and water came out there also. The top was now closed, the fire was urged to a much higher temperature, and the bottom of the boiler showed a dull redness, and when the lower cock was opened nothing but steam issued, this cock being shut, the upper one was opened, and water only issued evidently showing that all the water was at the top of the tube. The fire was now urged to a much greater intensity, and the bottom became red hot, and when the cock was opened nothing was heard, neither was steam seen to issue; but Mr. Gordon thought he smelt hydrogen, which was undoubtedly the case.

If my countrymen would repeat this experiment they would be perfectly satisfied of the correctness of what I have formerly stated. I have been extremely fortunate in meeting, in London, with Professor Hare, of Philadelphia, who is one of the investigating committee of the Institute. Dr. Hare, although one of the committee, has not taken part in any of the experiments, having made up his mind that it was useless to attempt to prevent explosions by any regulations which can be practically enforced. The Doctor's remedy is to prevent mischief when explosions do actually take place. This has always been my aim also, although our two systems are entirely different; mine has been to modify the boiler so as to prevent the possibility of explosions properly so called, bursting being the only result of a pressure of steam too high for the strength of the boiler.

It affords me great pleasure to have had such a man as Dr. Hare to witness these new facts which throw so much light on the science of steam. I shall conclude this paper by requesting my American friends to suspend their opinion of the report of the Franklin Institute (so far as I am concerned) until the return of Dr. Hare, who I hope will take the very machine used in this country to show the fact. JACOB PERKINS.

Physical Science.

Experiments on Solar Light. By JOHN W. DRAPER, M. D. Professor of Chemistry, Hampden Sidney College, Va.

(Continued from Vol. XIX., p. 479.)

43, *Action of vegetable leaves on carbonic acid.* 44, *Analysis of gas evolved by non-luminous heat.* 46, *Analysis of gas under ordinary circumstances.* 47, *Plants evolve nitrogen, and not oxygen gas, in the sunshine.* 52, *Effect of stopping the chemical rays.* 54, *Identity of the primary and resulting volume of the gas.* 58, *Non-oxygenation of phosphorus.* 62, *Decomposition of salts of silver.* 64, *Fundamental trial.* 65, *Discovery of the diffraction of the chemical rays.*

43. ACTION OF VEGETABLE LEAVES ON CARBONIC ACID.—The evolution of gas, depending, therefore, on the rays of heat, we are next led to inquire, whether the chemical rays affect the operation in any manner. To understand this, I exposed a quantity of boiled water, which had been suffered to cool in vacuo, to carbonic acid gas, of which it absorbed a certain amount. A portion of this water was placed in the focus of the brass mirror, 39, and was there acted upon, by the non-luminous rays; its temperature never exceeded 40° Fah. In a short time the pine leaves commenced giving off gas very copiously, and continued to do so; but it was found on trial, that nearly the whole of it was absorbed by lime water, and, that no decomposition had occurred. Therefore, though rays of non-luminous heat are competent to cause the evolving of gas, they are not able to cause decomposition.

44. The record of an analysis, will place this effect in its true light; care being taken that the water should be impregnated with pure carbonic acid gas, and the leaves recent, when a sufficient quantity was evolved, 39 measures were taken, of which caustic potassa absorbed 34. Hydrogen gas being then added, a diminution to the amount of 4 volumes was produced by a platinum ball, the remaining gas was proved to be nitrogen. The composition of this gas was therefore,

Carbonic acid, . .	34.00
Nitrogen, . .	3.67
Oxygen, . .	1.33
	39.00

It is proper to observe that a change very evidently takes place in the structure of the vegetable leaves, their colour becoming of a dirty brown, and all their greenness lost. Whether it is a change of their acting tissue, which hinders decomposition, or whether there is some peculiarity in the constitution of non-luminous heat, which incapacitates it from producing those effects which result from caloric radiating from highly incandescent bodies, I shall proceed to discuss.

45. Let us first consider what is the action of an ordinary unchanged sunbeam, on carbonic acid in solution, and in contact with vegetable matter. A wide distinction is here to be made, between common spring water, such as pump water, and water charged with carbonic acid *only*; the former, contains a compound of oxygen and nitrogen, isometric with protoxide of nitrogen; but the protoxide is a chemical compound, having its two volumes of nitrogen compressed into one, whilst this is a capillary compound, existing with an almost insensible condensation. The process of evolving gas

from spring water, and from carbonated water, is essentially different, the former taking place by an exaltation of temperature, occasioned by the impinging of radiant heat, no kind of decomposition at all going on; but the latter is accompanied by a true decomposition, due to the presence of vegetable matter.

46. This case will be better understood by an analysis of the gas given off from carbonated water. A certain volume of water, had its carbonic acid and all other gaseous impurity expelled, by long continued boiling; it was then rapidly cooled by refrigeratory measures, and impregnated with pure carbonic acid gas; being introduced into a mattrass, (plate I. fig. 16,) with a bunch of pine leaves, the neck of the mattrass dipping under the surface of some mercury contained in a cup, so as to cut off communication with the atmosphere, it was exposed to the sun, the day being very favourable, clear, and hot: 47.50 measures of the gas evolved, were taken; a piece of caustic potassa absorbed 3.50 measures of carbonic acid, the remainder being 44.00 measures; 90 measures of hydrogen were added thereto, making the full volume 134.00 measures; a platinum ball reduced this to 67.00; indicating 22.33 of oxygen, there remaining of nitrogen 21.67. The composition of this gas was, therefore,

Oxygen, . .	22.33
Nitrogen, . .	21.67
Carbonic acid, .	3.50
	47.50

To prove that the remainder here spoken of was really nitrogen, one hundred volumes of the original gas were taken, and the electric spark passed through it; there was no diminution in the volume, nor any carbonic acid gas generated; it could not therefore be carbonic oxide, hydrogen gas, nor any of the carburets of hydrogen; it possessed, moreover, all the negative qualities of nitrogen.

47. But, the solution was composed of carbonic acid and water; great care having been taken to cut off all access from the atmosphere during its preparation, and also during its exposure to the sun, for fear of a capillary interchange of the carbonic acid with the gaseous elements of atmospheric air. None such had occurred. From what source then came the large amount of nitrogen gas evolved? the only elements within the mattrass were carbon, hydrogen, and oxygen, yet here a large amount of nitrogen was found, which could have come from no other source than the pine leaves.

48. This fact, furnishes a clue to the mystery of the decomposition of carbonic acid, by vegetable matter. A compound expressed by the formula $c + 2O$, is exposed under the circumstances detailed; at the completion of the experiment, the constitution of the gaseous elements is expressed by $C + N$. We therefore find, that the vegetable leaves had absorbed one volume, whose composition is expressed by $c + 6$, and had given in exchange one volume whose symbol is N. Or, to speak without symbols, in this experiment the pine leaves absorbed one measure of carbonic oxide, and gave in exchange for it one measure of nitrogen, and the resulting gas contained therefore, half its volume of nitrogen, and half of oxygen, united without sensible condensation.

49. Hitherto it has been supposed by chemists, that when vegetable leaves were placed in carbonated water, they absorbed the carbon and caused the oxygen to be evolved. Vegetable physiologists, botanists, and others, have raised a great many theories upon this fact, which, however, a long

course of experiments assures me are without any foundation. There is no truth in the idea, that plants absorb carbonic acid, and assimilate carbon, and evolve oxygen. On the contrary, they actually evolve nitrogen, and the decomposition of carbonic acid, though remotely brought about by the action of the solar ray, is mainly due to the complex play of affinities of the elementary constituents of the plants.

50. I will here give another example in point, substantiating the same fact under different circumstances. Carbonated water that had been exposed with due care to the sun for two days being provided, 25.75 measures of the resulting gas were taken, and found to contain 1.25 of carbonic acid; for caustic potassa diminished them to 24-50. Next, 31.50 measures of hydrogen were added, making in all 56.00, and a platinum ball being introduced, there remained 7.50 indicating 16.16 volumes of oxygen, and 8.34 of nitrogen, the composition of the gas being therefore,

Oxygen, . .	16.16
Nitrogen, . .	8.34
Carbonic acid, .	1.25
	25.75

Allowing for unavoidable errors of manipulation, the formula will stand $26 + N$; the leaves therefore had absorbed a compound of oxygen and carbon, whose composition is expressed by the formula, $3.\ vap.\ C + O = 1.$ that is, condensed into one volume, and had rendered up one volume of nitrogen in return, being of course the same amount; the resulting gas was therefore one-third nitrogen, and two-thirds oxygen, united without sensible condensation.

51. If any further proof was required, that the evolution of nitrogen by the plant, is an essential part of this decomposition, it is furnished by the results of an experiment, in which spun glass was used to replace the pine leaves. This arrangement, though exposed to the sun under the most favourable circumstances, will not evolve any gas, but on passing into it a leaf, no matter how small, decomposition at once commences, because the requisite quantity of nitrogen is given off.

52. A box *a*, *b*, *e*, *e*, of a cubical shape, fig. 1, plate I., and nearly 12 inches in each of its dimensions, had one of its sides taken out, and replaced by a trough *k k* of suitable size, consisting of two glass plates, cemented at a distance of $\frac{1}{4}$ inch from each other. This trough was filled with a solution of bi-chromate of potassa; one of the sides of the box was hung on hinges *e e*, as a door, for the sake of obtaining access to the interior. Within this little chamber, a mattrass filled with carbonated water, and enclosing a bunch of pine leaves, its neck dipping beneath the surface of some water in a cup, was shut up, and exposed to the sun's rays, which, passing through the trough, impinged upon it. In a short time, air bubbles were copiously given off, and when a sufficient quantity was obtained for analysis, its constitution was determined. The following is selected from a number of analyses, being probably the most correct, and very nearly the mean.

Carbonic acid, . .	16.00
Oxygen, . .	8.16
Nitrogen, . .	4.84
	29.00

We here remark the existence of a far larger proportion of carbonic acid, but the relative proportion of the oxygen and nitrogen is still observed with tolerable accuracy, the deviation may be satisfactorily referred to disturbing causes. The greater amount of carbonic acid, as compared with sections 46 and 50, may likewise be due to the higher temperature of the arrangement when shut up in a close box, where currents of air, or other cooling agents, could not have free access to it. Or, it may hereafter be found, that there are chemical rays of *different colours*, as it were; or, more strictly of different refrangibility, and absorbability, and that those which find a passage through bi-chromate of potassa, can cause the decomposition of carbonic acid, though they cannot blacken chloride of silver. The doctrine that chemical rays are nothing more than undulations of an elastic medium, whose waves vary in breadth, I shall endeavour to support; each of these kind of waves, being competent to bring about changes peculiar to itself; or, adopting another hypothesis, that they are particles whose axes perform certain oscillatory movements. Not in this place, however, to anticipate what I have to offer on these matters; I shall continue to use the term *chemical rays*, as expressing those which blacken chloride of silver; and these, I say, are not engaged in the decomposition of carbonic acid.

53. From the first observations made on the decomposition of carbonic acid by Priestley, this subject has afforded much scope for chemical speculation. Count Rumford examined it successfully, but wanting means of accurate gaseous analysis, and above all not understanding the doctrine and laws of interchange through tissues, his conclusions are devoid of that degree of precision, which the advance of chemistry, in all its departments, enables us to attain. The conclusion to which these earlier philosophers came was, that plants had the power of absorbing carbonic acid, and rendering oxygen in return, by elaboration from their vessels; and this they regarded as the great means employed by nature to maintain the integrity of the composition of the atmosphere. A similar view has been taken of this subject by almost every philosopher, who has since examined it. Professor Burnet, to accommodate the theory to the observed facts, infers that plants exercise two functions, the one of breathing, the other of digestion, the latter only occurring during the stimulant action of the sunshine. This phenomenon is, however, unquestionably, one depending on the exalted capillary action of a tissue when radiant matter impinges on it; and the evolution of nitrogen or of some other gaseous or vaporous matter is, therefore, an essential part of the process.

54. The calculations of analyses made in the foregoing sections, involve the principle, that the volume of gas which remains after action is complete, is exactly the same as the volume of carbonic acid first operated on. The best method of proving this, is to take a tube, the diameter of which may be half an inch or upwards, which is graduated to inches and decimal parts. Fill it with water, from which all gaseous matter has been expelled by long continued boiling, place a few vegetable leaves in it, carefully removing any bubbles of air which may be attached to them, invert the tube in a vessel of water, and pass into it, as quickly as possible, a measured quantity of pure carbonic acid, and transfer it to a mercurial trough. This arrangement is seen in fig. 5, plate I. Conduct the experiment, first, in a cool dark place, absorption will rapidly go on, and in a short time all, or the greater part of, the carbonic acid will disappear, a column of mercury *e e*, rising in the tube to replace the gas. It is to be remarked, that it is not always easy to procure the entire absorption of all the gas, a little bubble remaining in the upper part of the tube, containing the impurities that may have existed in the gas, and also any remains of the carbonic acid, for the

amount absorbed depends upon several circumstances, as to the relative proportion of the volume of gas to the volume of water, the height of the mercurial column suspended in the tube, the temperature of the arrangement, &c. Then, on exposure to the solar rays, gas is copiously given off, the quantity continually decreasing until any further exposure ceases to evolve any more. On making the usual corrections for temperature and pressure, the aggregate of evolved gas will be found precisely the same as the volume first operated on.

55. Precisely I say, but this is with certain restrictions. Sometimes the volume is increased by an amount varying from .10 downwards; due chiefly to a certain amount of gas given off from the leaves extraneously; and partly to the capillary action of the whole system upon the elements of atmospheric air, which are transferred by slow degrees to the water operated upon, should there be a film of that fluid between the mercury and the sides of the glass tube; but, by making allowance for these disturbing actions, the proportion of equality will be found to be rigidly observed by the absorbed and the evolved gases.

56. We find, therefore, that the evolution and decomposition of carbonic acid by the solar ray, are due to that part of it exciting heat; that the chemical ray has no direct agency in the matter; it may bring about changes which to a certain extent complicate the phenomenon, but that it does not produce the abstraction of any compound of oxygen and carbon, from carbonic acid. Apart from the agencies exercised by the elements of the plant, agencies which are unquestionably of the least importance, the decomposition is remotely brought about by the action of radiant matter. But non-luminous heat, though capable of evolving gas, produces no change of its constitution; (section 44,) shall we then suppose that there is a difference in point of quality, between the heat given off by the bodies below ignition, and the heat of incandescent matter? Or, does the light itself aid decomposition. An experiment may be made, which appears to me, to bear directly on the answer which should be given to this query. Let a beam of light, fig. 3, plate I. two inches in diameter, pass through an aperture in the shutter A B, and fall upon any medium, *c d*, which should absorb a certain number of the rays of heat, as bichromate of potassa, which may be so diluted, as to absorb exactly 50 rays, out of every 100. Having, by means of a good thermometer, *g*, measured this, let the beam of light pass through a second trough *e f*, containing the same solution of the same strength, and its temperature again be taken, it will appear that the ray instead of losing half its heat, will contain nearly all of it, or in other words the second trough exerts no action on the passing beam. In an experiment, tried after the manner here indicated, the thermometer having shown a loss of 50 rays by the action of the first trough, fell only to 47, or gave a loss of 3 rays only, as the action of the second trough, an action to be referred, undoubtedly, to a degree of turbidness, which does exist to a small extent, in the clearest solutions; and also to the reflective and scattering action of the surfaces of the troughs. Now, the very same thing takes place in the case of light. A beam that has passed through a green or any other coloured glass, loses a large amount of its intensity, but if it pass through a second plate of the same tint, the second loss is entirely disproportionate to the former, and the reason of this is very apparent, for if the second plate had been of a different colour, the ray might have been much more affected, or even entirely extinguished. Delaroche made an identical observation in the case of non-luminous heat, for he proved, that

a plate of glass obstructed a large portion of the rays falling on it, but that a second plate allowed these rays to pass with far less loss. Now these experiments would lead us to conclude, that there are essential differences in radiant heat, analogous to the differences in light. The rays of heat given off by a cannister of hot water may be, to use an expressive solecism, *violet* heat, and a piece of transparent glass may be able to transmit *green* heat *only;* hence, in using two plates, the absorptive action of the first has the largest share in producing the phenomenon, the second transmitting nearly all which passed the first, an action identical with that of coloured glass on light. Bodies, as their temperature rises, emit more and more rays capable of passing through glass, simply because they become of that class over which the medium does not exercise an absorptive power.

57. The general conclusion which we are to draw from these researches is, that the decomposition of carbonic acid gas by vegetable matter, is a very complex phenomenon, due to the combined action of three forces, 1st the decomposing action of a tissue, 2nd to the impinging of radiant heat, 3d to chemical affinity, it being probable that any of these alone would be incompetent to produce this result. And, in the case of gas, such as oxygen being evolved from spring water, we are to refer the change to the ready decomposition of capillary compounds, compounds essentially distinct from chemical, and which can suffer decomposition by the force of capillary action. The colorific and the chemical rays have no influence in this latter case.

58. Non-oxygenation of Phosphorus. It is stated in the books, that Ritter in making observations on the slow combustion of phosphorus at common temperature, found that it emitted white fumes in the movable red ray of the solar spectrum, but in the movable violet, phosphorus in a state of oxygenation, was instantly extinguished. As a similar action is alleged to take place when the sun's rays shine on ignited carbon, it becomes desirable to understand the mode of action,—the original experiment of Ritter, was therefore repeated with a view to ascertain its accuracy. A cylinder of phosphorus, *a b* fig. 6, plate I., an inch long, and about one-sixth of an inch in diameter, was shielded from the action of aerial currents, by a glass jar. In front of the jar, an equiangular prism of flint glass was placed, so that the rays of a decomposed beam of light coming in through the shutter *c d*, could successively be thrown on the phosphorus which was placed horizontally in the jar; the beam of light also came nearly horizontally into the room, reflected by the arrangement already described. Situated thus, by turning the prism on its axis, any ray could be made to cover the phosphorus, the temperature in the shade being 72° Fah., a fine sheet of metaphosphoric acid, mingled with vapour of phosphorus, so thin as to be almost imperceptible except in certain positions, was observed to be rising from the cylinder, sometimes it would form a fine nebulous cloud, which hung for a moment on the phosphorus, and then rise gracefully in curled wreaths. The extreme mobility of this cloud was remarkable, even the warmth of the observer, by causing currents within the jar, would affect it, if the hand approached as at A, fig. 7, plate I., the phosphoric vapour came to the side of the vessel as it were to meet it, and then rebounded and circulated along the top of the jar. The size, position, and shape of this cloud when enveloped in the red light of the prism were deliberately marked, its motions were merely more capricious than when in the shade. And now, by turning the prism, the extreme *violet* ray was

brought upon it, but neither did its motion, or magnitude, or figure, appear in any wise changed.

59. The impression conveyed by Ritter's experiment is, that the chemical rays possess the faculty of hindering oxygenation. The negative conclusion here arrived at, might be due to local circumstances, and be referred to the action of the prism, as to its composition, to the state of the atmosphere, &c.; but no better success attended a variety of trials made on different days, and with prisms of crown glass, turpentine, and water. Trial was therefore made of absorbing media, a beam of light being made to pass at one time through a solution of sulphate of copper and ammonia, and at another through bichromate of potassa, but the condition of the phosphorescent cloud, was found to be too rough an estimate of the real action. A cylinder of glass A B, fig. 13, plate I., .75 inch in diameter and 3 inches long, was therefore fitted at its upper end with a stop-cock *a*, its lower extremity was closed air tight with a cork, through which an inverted syphon *b b* passed, each of its limbs being four inches long and the bore being $\frac{1}{6}$th of an inch; the outer limb was fitted with a scale. After having opened the cock *a*, a stick of dry phosphorus *e* was suspended in the cylinder, which was made very clean and dry, and the syphon being filled with water, was firmly seated in its place and the cock closed. Now as the phosphorus oxydized, the metaphosphoric acid was removed by the water present, and the level falling in the laternal limb, indicated what quantity of oxygen was consumed, and therefore the rate of combustion of the phosphorus. This was expected to give a more accurate estimate of any changes occurring in the phenomenon, and was accordingly applied to detect them.

60. A column of different coloured light, being made to pass at different times through the cylinder, so as to impinge on the phosphorus, attempts were made to ascertain the rate of combustion for each, as also for the white light of the sun, and in the shade. In each insulated experiment, the fluid in the gauge sunk with great regularity, more rapidly at first, but then more slowly, but the same regularity was not observed in different trials. At one time the phosphorus would consume with more than double the rapidity that it did at another, though to all appearance under identical circumstances. If the slow combustion of phosphorus be at all affected by the action of solar light, it is certainly not to that extent which Ritter supposed. So far from extinguishing, the violet rays do not exert any control over it, or if they do, it is to so small an extent that the most delicate arrangements fail to detect it.

61. It is possible however that atmospheric temperature may exert an influence on the result. During the trials here made, a thermometer in the shade has ranged from 70° Fah. to 82° Fah. At these points, the affinity of the combustible material for oxygen may be so exalted, that the action of any weaker force becomes masked. It is not stated what were the temperatures at which the alleged results were obtained. But it is most probable that the presence of extraneous matter has been the cause of all these variations. It is well known that certain compounds of hydrogen and carbon, in extremely minute quantity, will entirely put a stop to the oxidation of phosphorus, and during the course of these trials, I have had abundant reason to notice errors arising from this cause. By simply wiping out the cylinder with a linen cloth, which contained an almost imperceptible trace of spirits of turpentine, an erroneous result like that of Ritter was at once obtained.

Fig. 1.

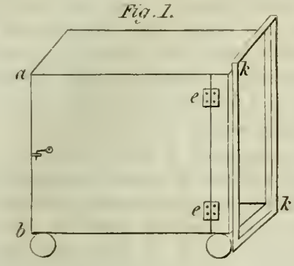


Fig. 2.

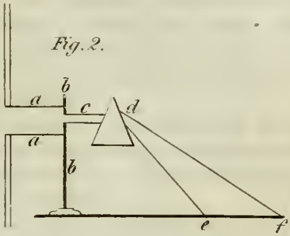


Fig. 5.

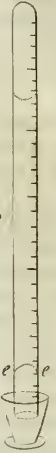


Fig. 3.

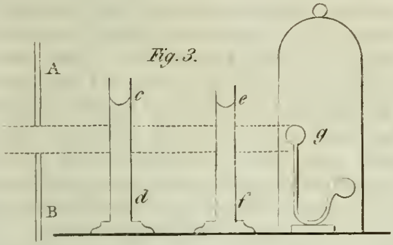


Fig. 4.

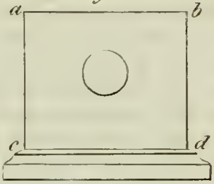


Fig. 6.

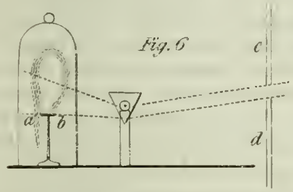


Fig. 7.

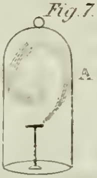


Fig. 8.

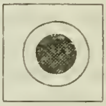


Fig. 9.

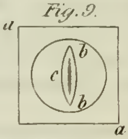


Fig. 12.

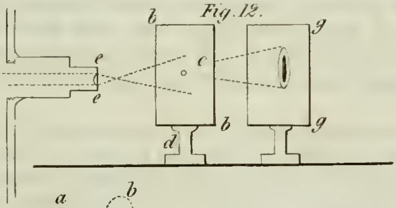


Fig. 13.

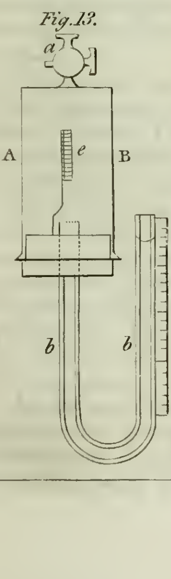


Fig. 10.

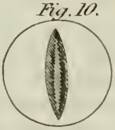


Fig. 14.

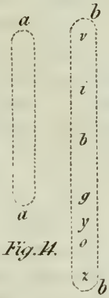


Fig. 15.

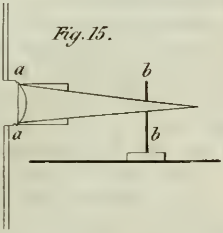
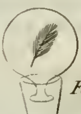


Fig. 11.

Fig. 16.



62. Decomposition of the Salts of Silver. Several of the salts of silver, undergo a remarkable change when exposed to the rays of light. The bromide, the chloride, and the nitrate, being very good examples; these, which are all white, become of a dark colour approaching almost to black, when exposed to the violet rays; it is stated that the bromide is most readily affected, yielding a brownish black colour.

63. If a piece of paper be soaked in a solution of nitrate of silver, and then dipped into a solution of bromide of potassium, it affords a very advantageous means of facilitating these experiments. The chloride may occasionally be substituted for the bromide of silver.

64. A beam of light, fig. 2 *a a*, entered a dark chamber horizontally, and was obstructed in its course by a plane metallic screen *b*, having a hole half an inch in diameter in it. The beam *c*, which passed through this aperture, fell upon a flint glass equiangular prism *d*, and was decomposed by it, the spectrum *e f* being received on the table, this spectrum was about three inches long. And now a piece of paper, imbued with bromide of silver, was placed to receive it, with the intention of ascertaining how far the discoloration would extend. In the course of five minutes, a very marked change had taken place, and on examination it was found that the deepest tint had been occasioned where the violet blended with the indigo rays; beyond this, even in the dark space beyond the spectrum, there was a stain, as also as far in the spectrum as where the green light merged into the yellow, an effect represented in fig. 14, *a a*, *b b*, being the spectrum, during this experiment the spectrum was kept stationary. Again, a column of light three inches in diameter, converging from a convex lens *a a* fig. 15, was intercepted by a screen of pasteboard *b b*, which had a circular aperture in it, half an inch in diameter, this screen was placed at such a distance from the focus, that the circular section of the cone of light was half an inch in diameter, and therefore passed exactly through the aperture; a piece of the prepared bromide paper, was then fastened on the back of the screen, so as to receive the condensed rays which passed the aperture. In a few moments a black spot appeared about the central parts of the paper, and at the end of the experiment there was an intensely black circle surrounded by a brown ring-like penumbra, as in fig. 8; the diameter of the black spot being three-quarters less than that of the aperture through which the light passed.

65. Diffraction of Chemical rays. Under certain circumstances, two ærial vibrations, each of which, if separately striking the organs of hearing, would produce a musical sound, may so interfere with each other as to produce an unmelodious rattling, or even silence. Also, two rays of light whose paths bear a certain relation to one another, instead of increasing each other's intensity, may have a directly opposite effect, and neutralizing each other, produce darkness. It becomes therefore a question, not only of mere curiosity, but one whose bearings are important, to find if the chemical rays emitted from the sun, when placed under similar circumstances, exhibit similar phenomena. For then analogy would lead us to know that it is possible for two rays of heat to be so situated with regard to one another, that instead of exalting the temperature of the body on which they fell, to lower it, or in other words to produce actual *cold*.

66. In my early trials for the solution of this question I met with many disappointments, but at last I fell upon an arrangement which yielded positive information. It is however an experiment requiring careful manipulation. A horizontal beam of light being projected into a room by the apparatus

heretofore so often referred to, at extremity *e e*, fig. 12, plate I. of the brass tube, a double convex lens of short focus was screwed, this brought the rays to a point at a distance of three-quarters of an inch from the lens, here they were obstructed by a metallic screen *b b*, having a round hole *c*, one-eighth of an inch in diameter, perforated in it. This screen, revolved about a vertical axis on a pillar *d*, so that it could be brought to any angle with the incident rays. The rays passing through the round hole *c*, were received on a white screen *g g*, at a distance of six inches. When the screen *b b* received the incident rays perpendicularly to its surface, then, of course, the image thrown on the screen *g g* was circular, but if the screen *b b* was made to receive these rays at an acute angle, then the image was lenticular. Under the last condition, the phenomena of diffraction is represented in fig. 9, plate I. where *a a* is the screen, *b b* the lenticular image cast on it, which is of bright white light, except at its central part *c*, where there is a dark image produced by the interference of the passing rays.

67. If in such an arrangement the chemical rays do not interfere with each other so as to neutralize their effects, chemical effects should be produced in every part of the image, even including its central part *c*; but if, on the other hand, these rays are obedient to the same laws as the rays of light, then in the central parts of the image no chemical effects should ensue; the problem is therefore reduced to the finding how any compound, changeable by these rays, will comport itself on the central and peripheral parts of such an image.

68. In place of the screen *g g*, a substitute was used consisting of two thin plates of mica, with a lamina of bromide of silver included between them, these were mounted in a little ivory frame *a b c d*, fig. 4, just in the manner that objects are usually mounted for the use of the microscope, and the lenticular image cast upon the bromide. After an exposure of five minutes, during which care was taken to keep the sun's place perfectly immovable, and also to avoid all local tremor, which might make the image traverse on the bromide, the result was very apparent, being as represented in fig. 11, of the natural size, the peripheral parts being of a deep brown, and the centre yellowish white. Viewed through a lens, the boundary line was not so sharp and distinct, but seemed to merge by insensible gradation into the unaffected part, as in fig. 10. The conclusion to be drawn from this result, possesses no common interest. For the same cogent reasoning, which applies in the proof, that light consists of undulations of an elastic medium, applies here also.

(TO BE CONTINUED.)

Progress of Practical and Theoretical Mechanics and Chemistry.

On the application of the Hot Blast, in the Manufacture of Cast-iron. By THOMAS CLARK, M. D., *Professor of Chemistry in Marischal College, Aberdeen.*

(Read 16th March, 1835.)

Among persons interesting themselves in the progress of British manufactures, it can scarce fail to be known, that Mr. NEILSON of Glasgow, manager of the Gas Works in that city, has taken out a patent for an important improvement in the working of such furnaces as, in the language of the patent, "are supplied with air by means of bellows, or other blowing apparatus." In Scotland, Mr. NEILSON's invention has been extensively applied to the making of cast-iron, insomuch that there is only one Scotch iron work where the invention is not in use, and in that work, apparatus is under construction to put the invention into operation. Apart from the obvious importance of any considerable improvement in the manufacture of so valuable a product as cast-iron, the invention of Mr. NEILSON would merit attention, were it only for the singular extent of the improvement effected, compared with the apparent simplicity—I had almost said inadequacy—of the means employed. Having therefore, by the liberality of Mr. DUNLOP, proprietor of the Clyde Iron Works, where Mr. NEILSON's invention was first put into operation, obtained full and free access to all information regarding results of trials of the invention in those works, on the large scale of manufacture, I cannot help thinking that an authentic notice of these results, together with an attempt to explain the cause of them, will prove acceptable to the Royal Society of Edinburgh. And that these results, as well as the cause of them, may be set forth with clearness I shall advert,

First, To the process of making iron, as formerly practised:

Second, To Mr. NEILSON's alteration on that process:

Third, To the effect of that alteration:

Fourth, To the cause of that effect.

I. In proceeding to advert to the process of making cast-iron, as formerly practised, it cannot here be necessary to enter into much detail in explanation of a process so long practised and extensively known, as this has been; nor, indeed, shall I enter into details farther than, to the general scientific reader, may be proper to elucidate Mr. NEILSON's invention.

In making cast-iron, then, the materials made use were three,

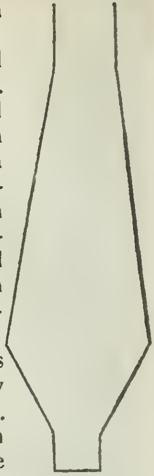
The Ore,
The Fuel,
The Flux.

The Ore was clay iron-stone, that is to say, carbonate of iron, mixed, in variable proportions, with carbonate of lime, and of magnesia, as well as with aluminous and silicious matter.

The Fuel made use of at Clyde Iron-works, and in Scotland generally was coke derived from splint-coal. During its conversion into coke, this coal underwent a loss of 55 parts in the 100, leaving 45 of coke. The advantage of this previous conversion consisted in the higher temperature produced by the combustion of the coke, in consequence of none of the resulting heat disappearing in the latent form, in the vapours arising from the coal, during its conversion into coke.

The Flux was common limestone, which was employed to act upon the aluminous and silicious impurities of the ore, so as to produce a mixture more easy to melt than any of the materials of which it was made up, just as an alloy of tin and lead serves as a solder, the resulting alloy being more easy to melt than either the lead or the tin apart.

These three materials—the ore, the fuel, and the flux, were put into the furnace, near the top, in a state of mixture. The only other material supplied was air, which was driven into the furnace by pipes from blowing apparatus, and it entered the furnace by nozzles, sometimes on two opposite sides of the furnace, sometimes on three, and sometimes, but rarely, on four. The air supplied in this manner entered near the bottom of the furnace, at about 40 feet from the top, where the solid materials were put in. The furnace, in shape, consisted, at the middle part, of the frustums of two cones having a horizontal base common to both, and the other and smaller ends of each prolonged into cylinders, which constituted the top and bottom of the furnace, as may be well enough conceived from the sectional sketch on the margin.



The whole of the materials put into the furnace, resolved themselves into gaseous products, and into liquid products. The gaseous products, escaping invisible at the top, included all the carbonaceous matter of the coke, probably in the form of carbonic acid, except only the small portion of carbon retained by the cast-iron. The liquid products were collected in the cylindrical reservoir, constituting the bottom of the furnace, and there divided themselves into two portions, the lower and heavier being the melted cast-iron, and the upper and lighter being the melted slag, resulting from the action of the fixed portion of the flux upon the fixed impurities of the fuel and of the ore.

II. Thus much being understood in regard to the process of making cast-iron, as formerly practised, we are now prepared for the statement of Mr. Neilson's improvement.

This improvement consists essentially in heating the air in its passage from the blowing apparatus to the furnace. The heating has hitherto been effected by making the air pass through cast-iron vessels, kept at a red heat. In the specification of the patent, Mr. Neilson states, that no particular form of heating apparatus is essential to obtaining the beneficial effect of his invention; and, out of many forms that have been tried, experience does not seem to have yet decided which is best. At Clyde Iron-works, the most beneficial of the results that I shall have occasion to state, were obtained by the obvious expedient of keeping red-hot the cast-iron cylindrical pipes conveying the air from the blowing apparatus to the furnace.

III. Such being the simple nature of Mr. Neilson's invention, I now proceed to state the effects of its application.

During the first six months of the year 1829, when all the cast-iron in Clyde Iron-works was made by means of the cold blast, a single ton of cast iron required for fuel to reduce it 8 tons $1\frac{1}{4}$ cwt. of coal converted into coke. During the first six months of the following year, while the air was heated to near 300° Fahr., one ton of cast-iron required 5 tons $3\frac{1}{4}$ cwt. of coal, converted into coke. The saving amounts to 2 tons 18 cwt. on the making of one ton of cast-iron; but from that saving comes to be deducted the coals used in heating the air, which were nearly 8 cwt. The nett saving thus was $2\frac{1}{2}$ tons of coal on a single ton of cast-iron. But during that year, 1830, the air was heated no higher than 300° Fahr. The great success, however, of those trials, encouraged Mr. Dunlop, and other iron masters, to try the effect of a still higher temperature. Nor were their expectations disappointed. The saving of coal was greatly increased, insomuch that, about the beginning of 1831, Mr. Dixon, proprietor of Calder Iron-Works, felt himself encouraged to attempt the substitution of raw coal for the coke before in use. Proceeding on the ascertained advantages of the hot blast, the attempt was entirely successful; and, since that period, the use of raw coal has extended so far as to be adopted in the majority of the Scotch iron-works.

The temperature of the air under blast had now been raised so as to melt lead, and sometimes zinc, and therefore was above 600° Fahr., instead of being only 300°, as in the year 1830.

The furnace had now become so much elevated in temperature, as to require, around the nozzle of the blow-pipes, a precaution borrowed from the finery furnaces, wherein cast-iron is converted into malleable, but seldom, or never, employed where cast-iron is made by means of the cold blast. What is called the *Tweer* is the opening in the furnace to admit the nozzle of the blowpipe. This opening is of a round, funnel-shape, tapering inwards, and used always to have a cast-iron lining, to protect the other building materials, and to afford them support. This cast-iron lining was just a tapering tube nearly of the shape of the blowpipe, but large enough to admit it freely. Now, under the changes I have been describing, the temperature of the furnace became so hot near the nozzles, as to risk the melting of the cast-iron lining, which, being essential to the *tweer*, is itself commonly called by that name. To prevent such an accident, an old invention called the *water-tweer* was made available. The peculiarity of this tweer consists in the cast-iron lining already described, being cast hollow instead of solid, so as to contain water within; and water is kept there, continually changing as it heats, by means of one pipe to admit the water cold, and another to let the water escape when heated.*

During the first six months of the year 1833, when all these changes had been fully brought into operation, one ton of cast-iron was made by means of 2 tons $5\frac{1}{4}$ cwt. of coal, which had not previously to be converted into coke. Adding to this 8 cwt. of coal for heating, we have 2 tons $13\frac{1}{4}$ cwts. of coal required to make a ton of iron; whereas, in 1829, when the cold blast was in operation, 8 tons $1\frac{1}{4}$ cwts. of coal had to be used. This being almost exactly three times as much, we have, from the change of the cold blast to the hot, combined with the use of coal instead of coke, *three times as much iron made from any given weight of splint coal.*

During the three successive periods that have been specified, the same blowing apparatus was in use, and not the least remarkable effect of Mr. Neilson's invention, has been the increase in efficacy of a given quantity of air in the production of iron. The furnaces at Clyde iron-works, which were at first three, have been increased to four, and the blast machinery being still the same, the following were the successive weekly products of iron during the periods already named, and the successive weekly consumption of fuel put into the furnace, apart from what was used in heating the blast:

	tons	tons	tons
In 1829, from 3 furnaces,	111 Iron	from 403 Coke,	from 888 Coal.
In 1830, from 3 furnaces,	162 Iron	from 376 Coke,	from 836 Coal.
In 1833, from 4 furnaces,	245 Iron		from 554 Coal.

Comparing the products of 1829 with the products of 1833, it will be observed that the blast, in consequence of being heated, has reduced more than double the quantity of iron. The fuel consumed in these two periods we cannot compare, since, in the former, coke was burned, and in the latter, coal. But on comparing the consumption of coke in the years 1829 and 1830, we find that although the product of iron in the latter period was increased, yet the consumption of coke was rather diminished. Hence the increased efficacy of the blast appears to be not greater than was to be expected, from the diminished fuel that had become necessary to smelt a given quantity of iron.

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* An incidental advantage attended the adoption of the water-tweers, inasmuch as these made it practicable to lute up the space between the blow-pipe nozzle and the tweers, and thus prevent the loss of some air that formerly escaped by that space, and kept up a bellowing hiss, which, happily, is now no longer heard.

On the whole, then, the application of the hot blast has caused the same fuel to reduce three times as much iron as before, and the same blast twice as much as before.

The proportion of the flux required to reduce a given weight of the ore, has also been diminished. The amount of this diminution, and other particulars, interesting to practical persons, will appear on reference to a tabular statement supplied by Mr. Dunlop, and printed as an appendix to this paper. Not further to dwell on such details, I proceed to the last division of this paper, which is,

IV. To attempt an explanation of the foregoing extraordinary results. Subsidiary to this attempt, it is necessary to discriminate between the quantity of fuel consumed and the temperature produced. For instance, we may conceive a stove to be kept at the temperature of 500° Fahrenheit, and lead, to be put into such a stove for the purpose of being melted. Then, since the melting point of lead is more than 100° higher, it is evident that whatever fuel might be consumed in keeping that stove at the temperature of 500°, the fuel is all consumed to no purpose, so far as regards the melting of lead, in consequence of deficiency in the temperature. In the manufacture of cast-iron likewise, experience has taught us, that a certain temperature is required in order to work the furnace favourably, and all the fuel consumed so as to produce any lower degree of temperature, is fuel consumed in vain. And how the hot blast serves to increase the temperature of a blast furnace, will appear on adverting to the relative weights of the solid and of the gaseous materials made use of in the reduction of iron.

As nearly as may be, a furnace, as wrought at Clyde iron-works in 1833, had two tons of solid materials an hour put in at the top, and this supply of two tons an hour was continued for twenty-three hours a day, one half hour every morning, and another every evening, being consumed in letting off the iron made. But the gaseous material, the hot air, what might be the weight of it? It can easily be ascertained thus: I find, by comparing the quantities of air consumed at Clyde iron-works, and at Calder iron-works, that one furnace requires of hot air from 2500 to 3000 cubical feet in a minute. I shall here assume 2867 cubical feet to be the quantity: a number that I adopt for the sake of simplicity, inasmuch as, calculated at an avoirdupois ounce and a quantity which is the weight of a cubical foot of air at 50° Fahrenheit, these feet correspond precisely with 2 cwt. of air a minute, or *six tons an hour*. Two tons of solid material an hour, put in at the top of the furnace, can scarce hurtfully affect the temperature of the furnace, at least in the hottest part of it, which must be far down, and where the iron, besides being reduced to the state of metal, is melted, and the slag, too, produced. When the fuel put in at the top is coal, I have no doubt that, before it comes to this far down part of the furnace, the place of its useful activity, the coal has been entirely coked; so that, in regard to the fuel, the new process differs from the old much more in appearance than in essence and reality. But if two tons of solid material an hour, put in at the top, are not likely to affect the temperature of the hottest part of the furnace, can we say the same of six tons of air an hour, forced in at the bottom near the hottest part? The air supplied is intended, no doubt, and answers, to support the combustion; but this beneficial effect is, in the case of the cold blast, incidentally counteracted by the cooling power of six tons of air an hour, or two cwt. a minute, which, when forced in at the ordinary temperature of the air, cannot be conceived of otherwise than as a prodigious refrigeratory passing through the hottest part of the furnace, and repressing its temperature. The expedient of previously heating the blast obviously removes this refrigeratory, leaving the air to act in promoting combustion, without robbing the combustion of a portion of the heat it produces.

Such, I conceive, is the palpable, the adequate, and very simple, explanation of the extraordinary advantages derived in the manufacture of cast-iron, from heating the air in its passage from the blowing apparatus to the furnace.

Marischal College, Aberdeen, Jan. 10, 1835.

APPENDIX.

TABLE *Showing the Weight of Cast-Iron produced, and the average Weight of Coals made use of, in producing a Ton of Cast Iron at Clyde Iron-works, during the years* 1829, 1830, *and* 1833, *the blowing engine being the same.*

Coke and cold air.			Coke and heated air.			Coal and heated air.		
1829	Weekly product of cast-iron by three furnaces.	Average of coals used to 1 ton of cast-iron.	1830	Weekly product of cast-iron by three furnaces.	Average of coals used to 1 ton of cast-iron.	1833	Weekly product of cast-iron by four furnaces.	Average of coals used to 1 ton of cast-iron.
	ton cwt qr	T cwt q		ton cwt qr	T cwt q		ton cwt qr	T cwt q
Jan. 7	137.18.2	8.12.1	Jan. 6	176.10.2	5. 2.2	Jan. 9	375. 8.0	2.12.3
14	148. 2.0	6. 9.2	13	181.12.2	5. 0·2	16	267.18.0	2. 4.0
21	148. 8.2	6.11.3	20	172. 5.2	5. 0.2	23	270. 7.2	2. 3.1
28	138. 9.2	7. 0.2	27	178. 7.0	4.19.0	30	250. 9.0	2. 4.0
Feb. 4	125.13.0	7.12.1	Feb. 3	164. 8.0	5. 4.0	Feb. 6	265. 3.2	2. 1.0
11	136.19.0	7·13.1	10	172.12.0	5. 4.0	13	202.10.0	2. 4.3
18	130.16.2	7·11.3	17	163. 9.0	5. 9.0	20	257. 1.0	2. 4.3
25	105.12.2	7·10.0	24	170. 1.0	5. 3.0	27	264. 0.0	2. 5.1
Mar. 4	101. 8.1	7·17.2	Mar. 3	154.19.0	5.10.3	Mar. 6	234.13.0	2. 5.2
11	111. 2.0	8. 2.2	10	154.16.0	5. 9.2	13	238. 7.2	2. 7.1
18	114.10.0	7. 6.2	17	151. 8.2	5. 9.3	20	205.13.0	2.10.2
25	110.14.0	8. 8.1	24	163.17.0	5. 5.1	27	217.14.0	2. 2.3
Ap. 1	111. 4.0	8. 7.2	31	163. 8.2	5.11.0	Ap. 3	220. 7.0	2.14.2
8	107. 7.0	8. 3.0	Ap. 7	147.10.0	5. 7.0	10	280. 9.2	2. 0.3
15	91.12.2	8.15.0	14	154. 9.2	5. 2.0	17	304. 7.0	1.17.3
22	85.13.0	9.13.0	21	163. 4.0	4.19.0	24	248.12.2	2. 3.0
29	91.14.2	9. 6.2	28	148.12.2	5. 4.0	May 1	245. 7.2	2. 6.0
May 6	92. 7.2	8. 8.2	May 5	162.10.2	5. 2.2	8	200.17.0	2. 8.0
13	94. 6.0	9. 2.1	12	149.13.0	5. 3.2	15	246. 4.2	2. 5.3
July 8	88. 4.2	8.16.3	19	162. 4 0	5. 5.0	22	219. 1.2	2. 6.0
15	91.13.0	8. 5.0	26	165. 7.2	4.18.3	29	231. 2.0	2. 8.0
22	97. 2.0	8. 2.1	June 2	160. 4.0	5. 2.2	June 5	335.16.0	2. 6 2
29	104.15.2	7.10.2	9	157.17.0	5. 1.0	12	232.10.0	2. 7.1
Aug. 5	106.17.2	7. 7.2	16	164. 0 0	4.17.3	19	271. 1.2	2. 1.0
12	93. 1.0	8. 6.0	23	149. 3.0	4.18.0	26	262. 3.2	2. 3.1
19	113. 7.0	8.18.2	30	162.16.2	4.16.3	½w. 30	122.16.0	2. 5.1
	2878.18.0	209.19.0		4215. 6.0	134. 6.2		6370. 3.0	58.18.3
Aver.	110.14.2	8. 1.1		162. 2.2	5. 3.1		245. 0.0	2. 5.1

The blowing-engine has a steam cylinder of 40 inches diameter, and blowing cylinder of 8 feet deep and 80 inches diameter, and goes 18 strokes a minute. The whole power of the engine was exerted in blowing the three furnaces, as well as in blowing the four, and in both cases there were two tweers of 3 inches diameter to each furnace. The pressure of the blast was 2½ lbs. to the square inch. The fourth furnace was put into operation after the water tweers were introduced, and the open spaces around the blowpipes were closed up by luting. The engine then went less than 18 strokes a minute, in consequence of the too great resistance of the materials contained in the three furnaces to the blast in its passage upwards.

Materials constituting a charge.

		cwt.	qrs.	lbs.
1829,	Coke,	5	0	0
	Roasted Ironstone,	3	1	14
	Limestone,	0	3	16
1830,	Coke,	5	0	0
	Roasted Ironstone,	5	0	0
	Limestone,	1	1	16
1833,	Coal,	5	0	0
	Roasted Ironstone,	5	0	0
	Limestone,	1	0	0

Trans. Roy. Soc. Edin.

An Experimental Inquiry into the Modes of Warming and Ventilating Apartments. By Andrew Ure, M. D., F. R. S.

The author having been consulted by the Directors of the Customs Fund of Life Assurance, on the mode of ventilating the Long Room in the Custom House, and deeming the subject one of great public interest, was induced to lay the result of his observations and experimental inquiries before the Royal Society. In this room, about two hundred persons are busily engaged in transacting the business of the Institution. All these persons are found to suffer more or less from ailments of the same general character, the leading symptoms of which are a sense of fulness and tension in the head, flushing of the face, throbbing of the temples, giddiness, and occasional confusion of ideas, depriving them of the power of discharging their duties, in which important and frequently intricate calculations are required to be gone through. These symptoms of determination of blood to the head are generally accompanied by coldness and languid circulation in the feet and legs, and by a feeble, and frequent, as well as quick and irritable pulse. On examining the air of the room by appropriate instruments, the author notices more especially three circumstances in which it differs from the external air: first, its temperature, which is maintained with great uniformity within a range of 62° to 64°; secondly, its extreme dryness, which, on one occasion, measured by Daniell's hygrometer, was 70 per cent.; and thirdly, its negatively electrical state, as indicated by the condensing gold-leaf electrometer. In all these qualities the air respired by the inmates of the room bears a close resemblance to the pestilential blasts of wind which, having passed rapidly over the scorching deserts of Arabia and Africa, constitute the *Simoom* of those regions, and are well known by their injurious effects on animal and vegetable life. To these noxious qualities is superadded, as in the air of all rooms heated through the medium of cast-iron pipes or stoves, an offensive smell, arising partly from the partial combustion of animal and vegetable matters always floating in the atmosphere of a town, and perhaps also from minute impregnations of carbon, sulphur, phosphorus, or even arsenic, derived from the metal itself. The author expresses his surprise that in the recent report of the Parliamentary Committee on the subject of ventilation, no reference is made to the methods employed for that object in factories, although they afford the best models for imitation, being the results of innumerable experiments made on a magnificent scale, with all the lights of science, and all the resources of the ablest engineers. He proceeds to describe these methods; and is then led to investigate the comparative efficiency, with a view to ventilation, of a draught of air resulting from a fire and chimney, and that produced by the rotation of a fan-ventilator. He shows that a given quantity of coal employed to impart motion to the latter, by means of a steam-engine, produces a ventilating effect 38 times greater than can be obtained by the consumption of the same fuel in the ordinary mode of chimney ventilation. Accordingly, he strongly advises the adoption of the former in preference to the latter: and inveighs against the stove-doctors of the present day, who, on pretence of economy and convenience, recommend the slow combustion of a large body of coke, by means of a slow circulation of air; under which circumstances, it is well known to chemists that much carbonic oxide, a gas highly pernicious to all who respire it, is generated; accompanied, at the same time, by a comparatively small evolution of heat. In order to obtain the maximum quantity of heat from a given mass of fuel, its combustion, he observes, should be very vivid, and the evolved caloric should be diffused over the largest possible surface of conducting materials; a principle which has been judiciously applied in several French factories. It has been proved that work-people employed in calico-drying rooms, heated according to the plan here reprobated, become wan, emaciated, and diseased; while in rooms in which the air is more highly heated by means of steam-pipes, they preserve their health and florid complexion.

L. & Ed. Philo. Mag.

Note on the Manufacture of Platinum, by M. Pelouze.

The method of Wollaston in the fabrication of platinum is only followed by those who make this metal an article of commerce. Chemists do not prepare malleable platinum for the requirements of their laboratories, and in their public lectures its preparation is never exhibited. M. Liebig is, I believe, the only one who manufactures it during his course. Although the method he follows is precisely that of Wollaston, and therefore presents nothing new in a scientific point of view, yet it may be alike useful and agreeable to chemists to retrace the steps of a process which is too much neglected and yet so easy of execution, that we may say there is no operation whatever more simple or expeditious than that of the manufacture of malleable platinum, made in the simple apparatus described below. It is a hollow cylinder slightly conical, one of the extremities of which is closed by a small, but very thick, metallic plate. After having decomposed, at as low a temperature as possible, the muriate of platinum and ammonia, the *froth* which is produced is separated by a piece of wood; with this and a little water a clear paste is to be made, and introduced into the cylinder. An iron piston is then introduced into the cylinder, and after having pressed it at first very gently for a minute or two, it is then compressed with the greatest possible force. An iron ring, by which the base of the cylinder is supported, being struck with a hammer, affords us facilities for getting at the piece of platinum which is thus formed. The platinum taken from the cylinder has already a high density, and brilliant metallic lustre. It is dried with a gentle heat; and, after having been exposed for a quarter of an hour to a white heat, it is rapidly withdrawn from the crucible, and receives a single blow of the hammer. It is then again exposed in the fire four or five times, and the number of the strokes from the hammer are only gradually increased. In less than half an hour the whole operation is finished; and it is so easy that the result is always certain. I now exhibit to the Academy a spatula and the blade of a knife of platinum, which I myself saw prepared in a few minutes at Giesen in M. Liebig's laboratory.

Edin. New Philos. Jour.

London Caoutchouc Company. Mr. Sievier's Patents.

The rapid increase in the use of India-rubber, or caoutchouc, as a material for the manufacture of various articles, has led to a speculation, having for its object the introduction of Mr. Sievier's patents to the public, under the title of the "London Caoutchouc Company." It has apparently met with a favourable reception, as the shares have been quoted in the market at a premium. The first patent, dated 1st December, 1831, and the second patent dated January 17th, 1833, are to secure the right of manufacturing cables, ropes, whale fishing and other lines, lathe and rigging bands, bags and purses; and for the manufacture of elastic goods or fabrics, very much in the mode so well distinguished by F. Peale, Esq., at page 109 of our last volume.

The third patent is dated February 7, 1836, and is for "An improvement in the means of dissolving and preparing caoutchouc, or India-rubber, for various purposes." This patent is solely for the application of Ammonia to the dissolving of India-rubber. The following is the process which the patentee describes:—

"I take caoutchouc, or India-rubber, cut into small pieces, and put them into any convenient vessel that may be closed at the mouth; I then fill the vessel with liquor ammonæ, so as to entirely cover the India-rubber; in a few months it will be dissolved, or its particles separated; I then put the solution so made into a still or large retort, and by the application of heat, nearly the whole of the ammonia will be distilled over in a gaseous form, and is to be taken up in the usual way by cold water; in that state it again becomes liquor ammonia; in this operation of distillation I prefer using a water-bath, as the India-rubber by that means cannot be subjected to a heat of more than 212° of Fahrenheit; the ammonia assumes a gaseous form at 130°. On separating the ammonia by distillation as above, the India-rubber is left held up in the water, and in that state may be applied for water-proofing cloths, or making solid masses of any form; by subjecting the solution to evaporation, any degree of consistency may be given to this solution by increasing or diminishing the quantity of water mixed with the previously dissolved India-rubber. * * * I claim as my discovery or invention the use or application of ammonia to effect the solution or separation in the manner herein described." Mec. Mag. G.

Fabrication of Beet Sugar.

From the *Receuil Industriel*, for November 1836, we make the following abstract in relation to the expenses and profits of the cultivation of beets and the manufacturing of sugar in France.

We shall take as an example the factories of our northern departments, where this enterprise has been longer and most profitably pursued. In this part of France a hectare of land suitable for the cultivation of beets, rents at a medium price for 70 francs a year. The occupier has to pay a duty of 12 francs per hectare, so that the soil of each hectare costs him 82 francs.

The expenses of seeding and culture including manure, labouring, weeding, and gathering the plants, may be estimated at 300 francs; hence a hectare of beets costs in these departments about 382 francs. The produce from this quantity of ground is from sixty to eighty thousand weight of roots. Taking 76 as a medium we find that the manufacturer who is also the cultivator of his beets, pays for them about 5 francs 45 centimes per thousand. Although in some favourable circumstances from 8 to 8½ per cent. of sugar has been obtained from the beet, it would not do to estimate the ordinary product at more than 5 per cent. At this rate the thousand of beets would yield 50 demi kilog. of sugar, and consequently the produce of a hectare would be 3,500 demi kilog. of sugar.

The cost of fabrication, including animal charcoal, fuel, manual labour, wear and tear of machinery, office duties, &c. amounts at least to 750 francs to bring to a crystaline state the sugar of a hectare of land. Hence the 3,500 lbs. of sugar which the hectare produces comes, altogether to 1132 francs, which makes a pound of sugar cost 32 centimes; but from this we may deduct the value of the molasses (about two per cent. of the weight of the beets) and of the residue of the pressed roots, (fifteen per cent. of the whole) which may be reckoned at eight centimes—so that the pound of sugar stands the manufacturer at the cost of twenty-four centimes.

The mean value of a hundred weight of sugar is 55 francs, and if these were the only considerations in the cost and produce, the profits would be enormous; but we must take into account the capital necessary to be embarked in the operation. Now it is estimated that a factory adapted to the working of five millions of pounds of beets, would require in the cost of land, buildings, and machinery, an outlay of 150,000 francs. The interest of this sum at 10 per cent. divided by the 250,000 pounds of sugar obtainable from the above quantity of roots, would come to 6 centimes per pound, which extends the cost to 30 centimes or 30 francs the hundred. This would yield a profit on five million of roots, of 60,000 francs a year; but such a profit requires an extensive fabrication. The advantages to those who operate on a small scale are much less in proportion. Nevertheless there have been, of late years, in the northern departments, small manufacturers, who perform the labour within themselves and do it with advantage. We may cite among those who work in a small way, a John Joseph Lecerp who keeps a small inn at Onain near Valenciennes. By means of a very simple apparatus he succeeds in making in one season about a thousand pounds of sugar. A single room fourteen feet square, contains his clarifying and evaporating vessels. His wife's lye-tub serves for a refrigerator. The only decent instrument which he possesses is a rasp which makes 400 revolutions in a minute and which is worked by his children. His sugar is of good quality, and he has even obtained the medal of the Agricultural Society, by way of encouragement.

The imperfect means which these small fabricators employ, only allows them to obtain 3 or at most 4 per cent. of sugar from their roots, and they find this advantageous only by the exemption which they enjoy from taxation. Even the show of an attempt in the Chamber of Deputies last season to extend the tariff, induced some of them to give up, and among those was Joseph Lecerp. Should such a law be passed, only rich manufacturers and very large establishments can be successful, and in this state of things it is next to a certainty that foreigners would appropriate to themselves a fabrication which they have studied among us, and would eventually supply us with the sugar of their production which would more than rival our indigenous manufactory. G.

Purification of Tar.

Tar contains a certain quantity of pyroligneous acid from the wood which produces it, and which in time occasions the destruction of cordage by thickening, in the manner of pitch, as soon as the oily portions have disappeared. This change prevents the threads from stretching and of course they must break, although they may still be sound. The acid should therefore be carefully extracted from the tar. This is effected by washing it in clean lime water. They are to be well agitated together in any suitable reservoir, by a convenient instrument of any kind, changing the lime water frequently, and continuing the operation until a good test paper indicates a freedom from all acidity.

Tar thus purified is a good preservative of wood, which it defends from decay much longer than common tar, because the acid attracts moisture from the air and retains it in the fibres of the wood; but purified tar repels moisture and thus acts as a preservative. G.

Jour. des Conn. Usuelles.

Acetiferous Lamp.

A very concentrated vinegar may be produced by the process of Dobereiner. Take a small glass vial and fill it with absolute alcohol. Introduce into its neck a small glass funnel, into which press some spongy platina and a thread of cotton, which dipping into the alcohol draws it to the platina. The latter acting on the air, the oxygen is absorbed and the alcohol converted into very strong vinegar, which may be collected by placing over the vial a bell glass resting on a glass dish. Dobereiner recommends the following method for preparing a very energetic platina. Make a kind of paste with ammoniacal muriate of platina and ammonia; put it into a platina crucible well covered and expose it to the action of fire. Ibid.

Progress of Physical Science.

On the Causes of the Tornado, or Water Spout. By R. Hare, M. D., &c.

In July 1835, I visited the scene of the tornado, which had in the previous month produced so much damage in and near New Brunswick, New Jersey, and heard it described by various witnesses, and have likewise been edified by the observations made respecting its effects by professors Henry, Torrey, Johnson and other sagacious and learned observers, and especially those of my friends, professor A. D. Bache, and Mr. Espy. Probably in no other instance have the effects of a tornado been so faithfully and skilfully traced, ascertained and registered. Professor Bache regularly surveyed the path of the devastating agent, and ascertained the bearings of the various bodies prostrated by it, so as to make several accurate plots. From an examination of these, the proximate causes of the changes effected, are those of a vertical current at the centre, or axis, of the tornado, and of a horizontal conflux of the air towards that axis from the surrounding space. Some trees appear to have been thrown down on the approach of the hiatus, both directly in front of it and on either side; some fell at right angles, others obliquely to the path. Hence they were found to have a great variety of bearings, but always pointing towards the path. The time of their falling, and consequently the direction agreeably to the observations of professor Bache, appear to have been determined not only by the extent of the force to which they were exposed, but likewise by the strength of their roots, or the degree of protection afforded them by other bodies, trees or houses for instance. On these accounts, neighbouring trees, falling at different times, had different bearings; but that they all fell towards the point occupied by the axis of the tornado at the time of their overthrow, appears to be consistent with the facts. In one instance, both professor Bache and Mr. Espy observed that the post of a frame building, being dislodged from the stone on which it rested, was first moved towards the path of the tornado in one direction about eighteen inches, marking its course by a furrow in the ground, and afterwards moved in another direction, nearly at right angles to the former, leaving a similar indication of the course in which it had moved. Intermediately between the time when the tornado bore in those directions, the frame was protected by a house.

While the phenomena above described sufficiently indicate the existence of a horizontal conflux of the air, that of a vertical force was demonstrated by the transportation of the debris of the houses and trees, as well as lighter bodies, to a great distance. A lady's reticle was carried seven miles from New Brunswick, and a letter twenty miles. The piece of timber, technically called the plate, on which the rafters of the roof of a church in New Brunswick rested, was carried nearly a quarter of a mile, and lodged in some trees beyond the Raritan. The fields, on the other side of that river, were strewed with shingles torn from the houses in the town.

After maturely considering all the facts, I am led to suggest that a tornado is the effect of an electrified current of air, superseding the more usual means of discharge between the earth and clouds in those sparks or flashes which are called lightning. I conceive that the inevitable effect of such a current would be to counteract within its sphere the pressure of the atmosphere, and thus enable this fluid, in obedience to its elasticity, to rush into the rarer medium above.

It will, I believe, be admitted, that whenever there is sufficient electricity generated to afford a succession of sparks, the quantity must be sufficient, under favourable circumstances, to be productive of an electrical current; and that light bodies, lying upon one of the electrified surfaces, may be attracted more or less by the other.

The phenomena of the rise and fall of electrified pith balls, called electrical hail, sufficiently justify this last mentioned statement; while the continuous stream is illustrated by the electrical brush, or the blast of air produced by a highly electrified point.

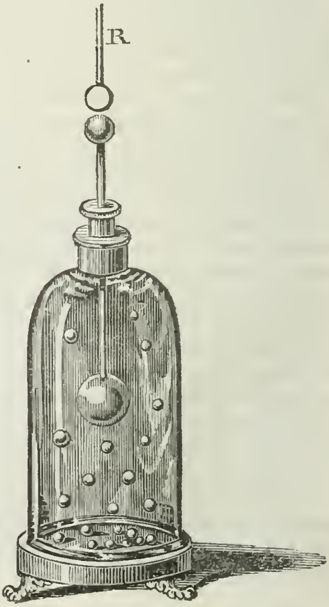
It will also be conceded, that thunder and lightning are caused by discharges of electricity between the earth and clouds, analogous to those of a Leyden jar or coated pane; the air performing the part of an electric in place of the glass, while the cloud acts as a coating.

It follows that the phenomena above mentioned as liable to arise between oppositely electrified bodies, may be expected to take place between the clouds and the earth, with effects as much exceeding those produced by human agency, as the snap and spark of an electric battery are exceeded by thunder and lightning. If in the one case pith balls and other light bodies are lifted; in the other, water, trees, houses, haystacks and barns may be powerfully affected.* If from a point electrified by a human contrivance, a blast of air may be induced; it is assuredly not unreasonable to ascribe to the analogous electrical apparatus of nature, aided by the elasticity of the air, a vertical hurricane. It was under the well founded impression that lightning may be superseded by a current, that we have been instructed by Franklin, to surmount our lightning rods by metallic points, by which electrical discharges from thunder clouds are expected to be conveyed to the earth gradually, which might otherwise pass in sparks of lightning of a formidable magnitude.

If, then, it be demonstrated that a continuous discharge of electricity may become the substitute for lightning, and that within the sphere of the discharge the air may be so lifted as to counteract its gravity; it is in the next place only necessary to advert to facts perfectly well known, in order to point out a cause of acceleration sufficient to account for the well known violence of the tornado.

At the height of fifteen miles, the air has been ascertained to have less than one-thirtieth of the density of the stratum next the earth. Of course this substratum would exercise a force nearly equal to the atmospheric pressure, or about fourteen and a half pounds to the square inch, in order to attain the space occupied by the rare medium, to which allusion has been made. It follows that if the weight of the superincumbent air were removed or counteracted, that the inferior stratum would rise with explosive violence.

While the air is thus carried upwards by the concurrent influence of electrical attraction, and the reaction of its own previously constrained elasticity; other bodies are lifted, both by electrical attraction, and the blast of air to which it gives rise. Hence houses within the sphere of the excitement are burst by the expansion of the air which they contain,



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* This figure affords an illustration in miniature of the rise and fall of bodies situated between oppositely electrified surfaces, which, in the gigantic operations of nature, are conceived to be the exciting cause of the tornado. The phenomena represented by it are designated in Pixii's catalogue as "grêle electrique," and may be thus explained. A metallic rod supports one ball within the bell glass, another without, so as to be in contact with the knob of another rod R, proceeding from the conductor of an electrical machine in operation. The brass ball being by these means intensely electrified, attracts some of the pith balls which lie upon the metallic dish in which the bell is situated, and which should communicate with the cushions of the machine. As soon as the pith balls come into contact with the electrified ball, becoming similarly excited, agreeably to the general law they recede from each other and are attracted by the oppositely electrified dish. Reaching the dish, they attain the same electrical state as at first, and are, of course, liable to be attracted again.

their walls being thrown outwards, and their roofs carried away; while, by the afflux of the atmosphere requisite to the restoration of its equilibrium, trees, houses and other bodies are thrown inwards towards the vertical current, from before, as well as from either side.

When once a vertical current is established, and a vortex produced, I conceive that it may continue after the exciting cause may have ceased to act. The effect of a vortex in protecting the space about which it is formed, from the pressure of the fluid in which it has been induced, must be familiar to every observer. In fact, Franklin ascribed the water spout to a whirlwind produced by the concourse of the atmosphere to a given point. His hypothesis was, as I conceive, unsatisfactory, because it did not assign any adequate cause for the concentration of the wind, or for the hiatus which was presumed to be the cause. This deficiency is supplied, if my suggestions be correct.

One fact, of which I am myself a witness, cannot be explained without supposing a gyratory force. About six feet of a brick chimney, without being thrown down, were so twisted on the remaining inferior portion as to be left with its corners projecting.

I have hardly deemed it necessary to advert to the cause of the progressive motion of a tornado, since that would appear evidently due to the current of the atmosphere within which it may be created.

I believe that the electrical excitement which gives rise to atmospheric discharges of electricity, in whatever form they may occur, is usually ascribed to the chemical changes taking place in the atmosphere; especially the formation or condensation of vapour.

Another view of this subject has suggested itself to my mind. It is known that the atmosphere acts generally as an electric, while the earth acts as a conductor of electricity; and since the electric fluid passes through an exhausted receiver with great facility, it results that the rare medium which exists at a great elevation, is equivalent to another conductor. Hence it is evident that there are three enormous concentric spaces, of which that which is intermediate contains an electric, to which the others may act as coatings. When the tendency of the electric fluid to preserve an equilibrium is taken into view, I believe myself justified in the inference, that not only the space occupied by the globe, but the region beyond our atmosphere, or where the air is sufficiently rare to act as a conductor, must abound with electricity. Thus the atmosphere is situated between two oceans of electricity, of which the tension may often be different. Between these electric oceans, the clouds, floating in the non-conducting air, must act as movable, insulated conductors; and from the excitement consequent upon induction, chemical changes, or their proximity to the celestial electric ocean, must be liable to be electrified differently from each other, and from the terrestrial electric ocean.

The phenomena of thunder storms may arise, from the passage of electricity from one electric ocean to the other being facilitated by an intervening accumulation of the clouds, or in consequence of discharges from one insulated congeries of clouds to another through the earth.

The aurora borealis may arise from discharges from one ocean to the other, of electricity which, not being concentrated by its attraction for intervening clouds within air sufficiently dense to act as an electric, assumes the diffuse form which characterizes that phenomenon.

Falling stars may consist of electric matter, in transitu between one portion of the celestial electric ocean and another, tending to restore the equilibrium when disturbed. They may, in fact, consist of electric matter, passing from one mass of moisture to another; as it may be imagined that in an expanse so vast, in which the tension is so low, there may be a great diversity as respects the quantity of moisture existing in different parts. Indeed, it may be conceived that at times the clouds, insulated from each other, may make their reciprocal discharges through the region occupied by the celestial ocean.

I have been informed by my intelligent friend, Mr. Quinby, who resided for some time in Peru, at an elevation of fifteen thousand feet above the level of the ocean, that the clouds in that elevated region are far more electric than in the lower country of the same latitude; and that, on this account, it was considered as dangerous, at times, to travel in the "*sierras*," or table land. Possibly thunder storms are more frequent in warm weather, in consequence of the greater elevation which the clouds then attain, and their consequent approximation to the celestial ocean of electrity.

Consistently with the hypothesis which I suggested in my essay on the gales of the United States, the enduring rains which accompany those gales are attributed to the contact of an upper, warm, and moist current of air, with a lower current of the same fluid at an inferior temperature, and moving in an opposite direction. It would follow that, on such occasions, the electricity of the upper region would be diffused among the clouds within the upper stratum, without reaching those existing within the lower current. But in such cases neither stratum would be sufficiently insulated and restricted in its extent to transmit the electricity in a concentrated form, or to be liable to the intense excitement necessary to produce a tornado or lightning.

Trans. Am. Philos. Soc.

Facts and observations respecting the Tornado which occurred at New Brunswick, New Jersey, in June 1835, *abstracted from a written statement made by* JAMES P. ESPY, M. A. P. S. By R. HARE, M. D., Professor of Chemistry, in the University of Pennsylvania.

The tornado was formed about seven and a half miles west of New Brunswick, and moving at the rate of about twenty-five or thirty miles in an hour, terminated suddenly at Amboy, about seventeen and a half miles from the place of its commencement. It appeared like an inverted cone, of which the base was in the clouds, and the vortex upon the earth. It prostrated or carried off every movable body within its path; which was from two hundred to four hundred yards wide. Trees which were embraced successively within its axis were thrown down in a direction parallel to its path; those on either side always pointing towards some point which had been under its axis. Houses were unroofed, and, in some instances, unfloored; in others, their walls were thrown down outwards, as if burst by an explosion. There are two facts stated by Mr. Espy, and confirmed by professor Bache, which demonstrate fully the existence of an hiatus. In a house which was exposed to the vertical influence of the tornado, a sheet was lifted from a bed, and carried into a fissure made in the southern wall, which subsequently closed and retained it. The same result was observed in the case of a handkerchief, similarly fastened into a fissure in the northern wall. In some instances, frame buildings were lifted entire from their foundations. Joists and rafters were torn from a house and thrown down at the distance from it of about four hundred yards, and in a direction opposite to that in which the trees not lifted from the earth's surface, were prostrated. Of course lighter bodies, such as shingles, hats, books and papers, and branches and leaves of trees, were carried to much greater distances. There was no general rain, but hail and rain accompanied the fall of the other bodies. The tornado lasted, in any one place, for but a few seconds: the whole of the damage done at a farm having been accomplished, as the farmer stated, while he was passing from the front to the rear of his mansion, so that, by the time that he reached the back door, there was a perfect calm. Meanwhile, his house and barn were unroofed, and all the neighbouring trees thrown down. The noise which accompanied the phenomenon was by every witness described as terrific, being best exemplified by the rumbling of an immense number of heavy carriages. Every object in its path was bespattered with mud on the side towards that from which it advanced. Houses looked as if roughcast, and individuals were so covered with dirt as to be disguised.

Some thunder and lightning attended the tornado. Some trees, which resisted the onset, yielded subsequently; and hence were piled upon those which had fallen earlier. The weaker trees were undermost, and pointed in the direction in which the tornado approached; while the stronger were on the top, pointing in the direction in which it moved away.

Four different places were noticed, where all the trees lay, with their summits directed to a common centre. In the middle of one of these localities, the house was unroofed, and a handkerchief and sheet were lodged within the fissures in the walls, as already stated. The windows in the same house were all broken, and much of the glass thrown outside. From the evidence, Mr. Espy infers that the apparent height of the tornado was about a mile. He states that there were, on the same day, two other tornadoes about seventeen miles apart; and of which the nearest was about the same distance from that of New Brunswick. He conceives that the phenomena all concurred to demonstrate an "inward motion from all directions towards the centre of the tornado, and an upward motion in the middle." These statements of Mr. Espy are confirmed by professor Bache.

One fact of some importance has not been mentioned by Mr. Espy, which was observed by persons who were upon the ground during, or soon after, the catastrophe. I allude to the partial withering of the foliage of those small trees or shrubs, which, from their suppleness, were like the reed in the fable, neither uprooted nor overthrown. This unpleasant effect was perceptible when I visited the scene. Each leaf was only partially withered. As it would be inconceivable that mechanical laceration could have thus extended itself equably among the foliage, a surmise may be warranted that the change was effected by the electricity associated with the tornado.

Concluding Remarks by the Author of the Article.

I ought, perhaps, sooner to have acknowledged that I am aware it has often been suggested that water spouts might be caused by electricity; but the conjecture has not, as far as my information goes, been heretofore supported by any satisfactory explanation as to the mode in which such a tremendous power could arise from that source. That I am warranted in this impression, will, I trust, appear evident from the circumstance that two of the most distinguished among the late writers in the department of science to which the subject belongs, seem to admit, or to demonstrate, their inability to afford any explanation. I allude to Pouillet, and Despretz.

In his treatise on meteorology, Pouillet introduces two narratives respecting tornadoes, which were analogous in every essential point to that of

New Brunswick. The existence of an hiatus especially is proved by the allegation that the walls of prostrated houses were thrown down outwards. A labourer was first urged forwards, in the next place lifted, and lastly overthrown.

The learned and ingenious author concludes with these remarks:

"Comment cette puissance, quelquefois si prodigieuse, peut-elle prendre naissance au milieu des airs? C'est une question, il faut le dire, á laquelle la science ne peut faire aucune réponse précise. De toutes les conjectures vagues et hasardées, que l'on peut faire sur l'origine de ce météore, la moins invraisemblable est peut-être celle que le regarde comme un tourbillon d'une excessive intensité. Mais une discussion sur ce point nous semblerait prématurée; il faut multiplier les observations, et constater avec plus de précision toutes les circonstances de ces phénoménes."—*Elemens de Physique Experimentale et de Météorologie, vol. 2, p. 727.*

All the information respecting tornadoes afforded by Despretz is comprised in the following paragraphs, which I quote in his own words.

" *Trombe.* La trombe se montre en mer et sur la terre; tantôt elle semble sortir du sein de la mer, et s'élève jusqu'aux nuages; tantôt elle descend des nuages jusqu'à la terre.

" C'est une colonne d'eau cônique qui tourne sur elle-même avec une grande vitesse; elle a quelquefois jusqu'à plus de deux cents mètres de base. Elle est très-commune entre les tropiques: les navigateurs passent rarement près des côtes de Guinée sans en apercevoir plusieurs.

"Les trombes produisent des effets terribles; elles déracinent les arbres, renversent les faibles habitations, soulèvent les voitures, etc.

"On peut se faire une idée des trombes par les tourbillons de pouissiére qui se forment tout à-coup, en été, sur les routes, et qui tournent sur eux-mêmes avec une grande rapidité."—*Traité Elementaire de Physique, paragraph* 656, *page* 828, *par C. Despretz.*

In Nicholson's Journal, quarto series, London 1797, vol. 1, page 583, there is an interesting account of some tornadoes seen from Nice, illustrated by engravings, by M. Michaud, who appears to consider them as the effect of electricity, and infers that he could produce the phenomenon in miniature by the aid of a machine, as thunder and lightning are by the same means illustrated. This I have found to be erroneous, as far as my experience goes, and from a cause which is, agreebly to my hypothesis, quite evident. I mean the absence of the co-operating influence of the air when emancipated by electric attraction from the confinement arising from its own weight.

The theoretic remarks of Michaud are very brief, and, to me, scarcely intelligible, as he does not inform us in what way he supposes the electric fluid to operate.

I have understood, since I conceived my hypothesis, that Beccaria ascribed water spouts to electricity, but I have not had the advantage of learning by what reasoning he justified his inferences. However, should it appear that I have made, through the want of information, any undue claim to priority, I shall cheerfully do justice to any philosopher whose speculations I may have overlooked.

Trans. Amer. Phil. Soc.

On the Decomposition of Carbonate of Lime by Heat. By M. Gay Lussac.

It was long ago asserted that the calcination of calcareous rocks was favoured by the presence of water; and this opinion appears to have been admitted even by the greater number of those who were habitually employed in the preparation of lime. M. Dermas admits the influence of water as unquestionable and gives this double explanation of the fact, that it (the water) either acts upon the carbonate by forming an ephemeral hydrate, expelling the carbonic acid and taking its place until it is itself expelled by a red heat; or, that the water, being decomposed by the carbon employed as fuel, various gases are produced, one of which is carburetted hydrogen. This, reacting upon the carbonic acid of the carbonate, tends to convert it into carbonic oxide, and thus facilitates its separation from the carbonate of lime. Hence, limestone recently extracted and consequently still moist, must be more easily calcined than stone that is nearly dry. The greater number of limeburners are acquainted with this fact, and therefore they water the stone which has been long taken from the quarry, before they place it in the kilns.*

But the first of these two modes of explanation is inadmissable, since hydrate of lime is decomposed at a heat considerably below that which is essential to the decomposition of carbonate of lime under the influence of the vapour of water.

The second explanation, considering the circumstances of the combustion of lime kilns, does not appear to me to apply to the facts. I shall not, therefore, stop to examine it, but proceed to state the observations, which, if I am not mistaken, will furnish the true explanation of the influence of water in the calcination of calcareous rocks.

Having filled a porcelain tube with fragments of marble, I placed it over a furnace whose temperature was easily regulated. To one end of the tube was adjusted a glass retort containing water, and to the other end a glass tube for conveying the carbonic acid. The heat was first raised to the point necessary for decomposing the marble; and then, by accurately closing the ash-hole it fell to a dark red, and the carbonic acid ceased to be given out. At this juncture the water in the retort was made to boil, then the carbonic acid reappeared in abundance. When the vapour was intercepted the flow of gas instantly stopped and was restored only by readmitting the steam. The circumstances were thus successively varied, and with them the results themselves.

It appears thus demonstrable, that the vapour of water really favours the decomposition of carbonate of lime by heat, and that by its concurrence this decomposition takes place at a temperature inferior to that which is ordinarily required.

The action of the water appears to me purely mechanical. When carbonate of lime, exposed to heat, begins to undergo decomposition, there is formed around it an atmosphere of carbonic acid, which presses on the acid still in combination, so that, in order to become disengaged it must overcome the pressure of this atmosphere. Now this can take place only by a previous elevation of temperature,—or, by removing the carbonic acid and leaving a vacuum,—or, otherwise, by displacing it either by the vapour of water or some other elastic fluid, such as atmospheric air, &c.

This explanation is justified by the following experiment. I brought some carbonate of lime, in a porcelain tube, to a temperature a little inferior to that at which it began to decompose, and then I caused a current of atmospheric air to pass into the tube. The disengagement of carbonic acid immediately recommenced and continued in connexion with the current of common air,—stopped when it stopped, and was renewed whenever it was readmitted.

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* Dumas. Traité de Chimie, &c., II., 482.

It appears then to me, evident, that the influence of watery vapour, in the burning of lime is limited to the production, merely, of a vacuum for the carbonic acid, and thus preventing a pressure upon that which remains associated with the lime. With the presence of vapour a less elevated temperature is necessary to dislodge it; but still we must not overrate the importance of this vapour. The water in calcareous rocks is mechanically interposed between their particles; and, if we except a trifling portion which remains confined in the centre of pieces so large that the heat does not reach and evaporate it, a large portion of the water must evaporate without producing any useful effect, but on the contrary with a loss of fuel, before the stone has acquired the temperature needful to its decomposition.

I am convinced then that the vapour of water favours the calcination of limestone, but I am doubtful of the real advantage which it is supposed to produce, because there is no great difference between the temperature at which carbonate of lime is decomposed alone, and that at which it is effected with the concurrence of water. Besides, if the steam exerts only a mechanical action similar to that of atmospheric air, we see no important advantage it can yield over the aeriform current which incessantly penetrates the calcareous mass subjected to calcination.

The increased facility of the decomposition of limestone by aqueous vapour, or more properly, by a vacuum, ought not to remain an isolated fact. It may be regarded as an established principle, that when decomposition by heat, or by a chemical agent, must necessarily produce one or more gaseous elements, it may in general be favoured by retaining the substance in a vacuum, or by preventing the elastic fluids disengaged from pressing on it. And, reciprocally, that we may retard or entirely prevent decomposition by forming round the body the pressure of an elastic fluid, of the same nature as the one disengaged. It was thus, in the curious experiment of Hall, that carbonate of lime was brought into a state of fusion at a very high temperature, without undergoing decomposition, and the influence of an adequate pressure of carbonic acid.

Ann. de Chimie et de phys. Oct. 1836.

Remarks on the preceding article added by Dr. Hare.

It is due to justice to state that in the preceding observations and references Guy Lusac has been anticipated by Berzelius. This distinguished chemist informs us (Traite, vol. 2d, page 358) that calcareous carbonates are more difficult to decompose by heat in a covered crucible, than in one which is uncovered; but that by the introduction of a jet of aqueous vapour, or any gas of a different kind from that which is to be evolved, the evolution of the carbonic acid is effected with greater rapidity and less heat. In explanation of these facts, he alleges that the particles of the gas are less resisted by heterogenous particles than by its own.

Extract of a Letter from Mrs. Somerville to M. Arago, detailing some Experiments concerning the Transmission of the Chemical Rays of the Solar Spectrum through different Media.

In the account of the meeting of *L'Academie des Sciences* on the 21st of December 1835, (Compte Rendu, p. 508) it is stated that M. Arago, after having repeated what was most essential in those experiments by which M.

Melloni proves that the solar rays, while preserving all their luminous properties may be deprived of their calorific power, remarked, that there was another point of view in which the subject might be investigated. He said it would be important to inquire if the means employed by M. Melloni, or other analogous ones, would not enable us to deprive the solar rays of their chemical properties also; or, in other words, if, of the three properties which light possesses when it reaches us from the sun,—1st, That of illuminating; 2d, That of heating; and, 3d, That of destroying or exciting chemical combination, we could not separate the two latter, and retain its simple illuminating power. This experiment, remarked M. Arago, would probably lead to curious results, and I last week almost yielded to the temptation of undertaking the investigation. But, as possibly M. Melloni may have himself thought of it, though quite silent about it in his memoir, I think I had better not prosecute the subjects till after consulting the learned Italian philosopher.

The motives which I had in 1835, said M. Arago, at the meeting of the Academy on the 17th of October 1836, not to interfere in researches which so directly conducted M. Melloni to these beautiful discoveries, still subsist. I shall, therefore, abstain from stating some results to which I have arrived concerning the absorption, or the interception, of the chemical rays. Every one, however, will understand that the same reserve cannot be imposed upon Mrs. Somerville; and I cannot, therefore withhold the interesting experiments of this illustrious lady, from the Academy, and the public.

In my experiments, she remarks, I employ the chloride of silver, which Mr. Faraday was so kind as to prepare for me, and which, accordingly was perfectly pure and white. It was liquid, and might be spread uniformly over paper. Although this substance is exceedingly sensible to the action of the chemical rays, yet as we have no precise means of appreciating the changes of colour produced by their action, some uncertainty as to the result might remain were we to compare only those tints which differ but little from each other; but the results which I shall furnish on this occasion shall be chosen from among those which were in no degree doubtful.

A piece of glass, of a light pale green colour, which was perfectly transparent, and less than $\frac{1}{20}$th of an inch in thickness, did not permit any of the chemical rays to pass; after exposure for half an hour to a very hot sun, the chloride of silver behind the glass exhibited no change of colour whatever.

I have repeated this experiment with many pieces of green glass, which differed both in their tint and thickness, and I have always found that they were all nearly impervious so far as the chemical rays were concerned, and even after they had been subjected to the solar influence for a much longer period than that above stated. As M. Melloni has already found that glass of this colour arrests the most refrangible calorific rays, by associating his results with mine, we are led to conclude, that glass of this colour has the power of wholly intercepting the most refrangible portion of the solar spectrum.

Laminæ of mica, of a deep green colour, are also nearly impervious to the chemical rays; however, when they are very thin, and the solar action is continued for a very long time, then it appears that they do not completely arrest these rays. I fixed, with a little wax, to a sheet of paper which was covered over with the chloride of silver, a sheet of pale green mica from Vesuvius, the thickness of which was not more than the thirtieth of an inch, and I exposed the whole to the rays of a powerful sun; after a time the sheet of mica being removed, it was found that that part of the paper which it covered retained all its original whiteness, while the rest was wholly of a deep brown colour.

The same experiment has been tried with fine sheets of white mica. Six sheets of common white mica placed on each other did not intercept the chemical rays; the chloride of silver which they covered, at the end of an hour's exposure to the sun, had become quite brown. The same result was obtained after using a single plate of mica, which, however, was still thicker than all the others put together. This substance does not appear to present any obstacle to the transmission of the calorific rays.

These experiments led me at first to suppose that all green substances possessed this property: but I very soon found that this would be drawing too hasty a conclusion; for, having shortly afterwards tried the experiment with a very large emerald, the green of which was very beautiful, though not very deep, and the thickness of which was at least 0.35 of an inch, I found that it readily transmitted the chemical rays. Thus, the matter which imparts the colour to the green emerald has no action on the chemical rays, whilst that which imparts the same colour to glass and mica has great influence over them.

Rock-salt, as might be expected, possesses in a high degree the faculty of transmitting the chemical rays. Glass, too, coloured violet with manganese, and very deep blue glass, such as is common in finger-glasses, likewise very readily transmit these rays. The alteration in the colour of the chloride of silver very speedily takes place in spite of the interposition of a plate of blue glass of the deepest tint, and nearly a quarter of an inch thick.

Among the various substances which I have tried in these experiments, rock-salt and white glass, as also blue and violet-coloured glasses, are those which afford the maximum of permeability to the chemical rays; whilst the green shades of glass and mica present the minimum. Other bodies possess this property in intermediate degrees, and sometimes vary considerably, though the colour is nearly the same. Thus glass of a deep red colour allows very few of the chemical rays to pass, whilst garnet, of an equally deep colour, allows nearly the whole of them to pass. The white topaz, as well as the blue, the pale blue beryl, the cyanite, the heavy spar, the amethyst, and various other substances, transmit the chemical rays with great facility; whilst the yellow beryl does not, so to speak, transmit them at all, and the brown tourmaline as well as the green, have the property in so slight a degree, that I have failed in my attempts to polarize the rays under these circumstances, though I believe it might not be impossible, if thinner plates were used than I had it in my power to employ. In concluding, I may observe, that I purpose shortly to resume the prosecution of the subject.*

Edin. New Philos. Jour.

On the cause of the remarkable difference between the attractions of a Permanent and of an Electro-Magnet on soft iron at a distance. By the Rev. Wm. Ritchie, L. L. D., F. R. S., Prof. of Nat. Phil. Royal Inst., and in Univ. of London.

Attach a piece of soft iron, such as the lifter of a common horse-shoe magnet to one extremity of a slender delicate balance of light wood, and balance it by weights in a scale pan at the other end. Place a permanent horse-shoe-magnet below it and ascertain its attractive force by weights, both when in contact and when removed to different distances from the soft iron. Remove the permanent magnet and substitute a very *short* electro-magnet of equal lifting power. Remove it to the same distance as before and the attractive power will diminish very rapidly compared with that of the permanent steel magnet.

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* Would it not be interesting to ascertain by careful experiments, the relative effects of green and white glass shades upon plants, as practised by gardeners. G.

2. Instead of the short electro-magnet, substitute a very long one (of two or three feet long for example) and of equal carrying power; remove it to the same distances and ascertain its attractive power, and it will be found that its attraction for the lifter will *not* diminish so rapidly as that of the short one. The longer the electro-magnet becomes, the more does it approach to the character of the permanent magnet in all its properties.

3. Make the electro-magnet of *hard* iron or untempered steel and its power at a distance will be much greater than in the electro-magnet of soft iron.

These facts are accounted for by Dr. Forbes, in the following manner:

The perfect equality of *action* and *reaction* must be found to exist in this case as well as in every other in which *force* of any kind is concerned. The electricity which has been decomposed and arranged in the soft iron in the peculiar manner which constitutes magnetism, cannot decompose and arrange the electricity belonging to the lifter without suffering a corresponding *diminution*, and the more difficult the arrangement in the lifter so much greater will be the diminution of power in the electro-magnet. Again, if the electricity in the electro-magnet be easily arranged by the induction of the voltaic helix, it will be easily forced back to its natural state by the *reaction* of that belonging to the lifter. Hence it follows that when the *inducing* power of the electro-magnet is very great (which it is when the lifter is in contact with its ends) it will possess sufficient power to *vanquish* the *coercitive* force of the lifter, arrange by induction a large portion of the electricity of the lifter, and thus exhibit powerful attraction. When the lifter is removed to a certain distance, one-tenth of an inch for example, the power of the electro-magnet being much *diminished* in consequence of the distance, whilst the difficulty of overcoming the coercitive force of the lifter is *increased*, the effect will be very small compared with the former. For if the inducing power be only *equal* to the coercitive force of the lifter, no attraction whatever will take place; and hence the impossibility of magnetizing a large bar of steel tempered as hard as possible, by means of a small permanent magnet with a soft temper.

Now, if the coercitive force of the electro-magnet be increased, which is done either by employing a long magnet, or using hard iron or untempered steel, such a magnet will suffer *less* diminution by the *reaction* of the lifter in the case of increased difficulty of arrangement in the lifter, than in the case of the short electro-magnet of perfectly soft iron.

In the case of the permanent magnet of tempered steel, the electricity belonging to it was arranged with *difficulty*, and after repeated *touches* of another magnet, and consequently it will easily vanquish the coercitive power of a piece of soft iron, and induce a magnetic state upon it, whilst the peculiar arrangement of its own electricity will remain nearly *unchanged*. Hence its *attractive* powers will diminish *nearly* as the squares of the distances of the soft iron from its poles, or *imaginary* centres of accumulation, a law which cannot exist in the case of the electro-magnet the electricity of which is so easily put in motion round the elementary molecules of the iron by the reaction of the lifter. Lond. & Ed. Philos. Mag.

Progress of Civil Engineering.

On the advantage of using a mixture of Powder and Saw-dust in Blasting.

It has been frequently inquired, whether a mixture of powder with sawdust is preferable, in blasting, to powder alone; and the replies have been various, according to the influence of habit and prejudice, private interest, or want of perseverance in the necessary experiments. The saving of so expensive a material is, however, an important consideration in most mining operations, and although the increased effect appears unaccountable to many who have been used to avoid most carefully the mixture of any foreign substance with the blasting powder, yet this effect may, I think, be explained, simply by the fact, that the wider diffusion or greater looseness of the powder, in consequence of the mixture, gives rise to a more immediate and general ignition of the whole mass.

When the holes are charged with powder alone, the powder cannot all ignite at the same instant, and, consequently, a part of it is without effect, for if a new piece of board be placed before a hole charged with powder alone, grains of powder, which have not become ignited, will be found after the explosion on the board. In order to contribute in some degree towards the more general adoption of this highly useful method, I may be allowed to present to the mining public the results of my experiments, begun in December, 1818, and continued several months, which fully convinced me that mixed powder is, in every case, applicable and advantageous; and, since that period, I have directed this mixture to be constantly used in these mines. The saving I have found to be, on an average, two-fifths of the former annual consumption. The powder used here is from the Stollberg mills—is proof at thirty to thirty-two degrees, and of a middling grain. Before using, it is dried as highly as possible; the sawdust undergoes a similar drying, but without causing it to lose its natural colour, or approach a state of carbonization: in other respects it is in the same condition as when first brought from the saw mills. As the mixture, when packed in the ordinary powder-bags, does not continue in the same relative and equal position, in consequence of the component materials separating, by reason of the difference in their specific gravity, I make use of well tinned plate-iron powder-flasks, with a wide well-closed aperture, from which the men fill the cartridges by means of an iron or wooden spoon.

According to Mr. Varnhagen, the saving of powder in Brazil is three-fourths, which I think is hardly possible. I have made trials with this proportion, but no effect was produced; and I was therefore obliged to increase the proportion of the powder to two parts to one of sawdust in bulk, although, in many cases, I could only procure a satisfactory result by a composition of three parts of powder with one of sawdust. Not to offend the prejudices of the workmen, I added powdered charcoal to the sawdust, which answers the same purpose as the sawdust, but charcoal from soft wood is preferable to that from hard, since the former is more inflammable, spreads the powder more loosely, and does not explode in a regular tabular form, which would prevent the simultaneous and instant ignition of the grains of the powder. As it was the opinion of several authors that the evolution of gases, occasioned by the ignition of the sawdust, augmented the effect of the powder, I likewise made trials as to the correctness of this notion, using sawdust from fresh cut, from dry, and from green woods, and even moistening a pound of it with one-twelfth of its volume of clear water, and although the operation of filling and lighting was performed with the utmost possible dispatch, yet I noticed no augmentation in the effect produced. But even supposing this to be really the case, how many difficulties stand in the way of this method, since it would be exceedingly difficult, if not impossible, to fix the exact proportion of moisture. It is by no means an easy matter to induce the workman to act up to instructions, which his confined views prevent him from fully comprehending; and, indeed, the aversion to innovation is so great, that after the lapse of three years, I still find old miners who will rather purchase their own powder for blasting than use the mixture.—*Karsten's Archiv.* vol. v. p. 199. 1822.

Experiments for ascertaining the difference of consumption in using powder alone, or a mixture of equal parts of powder and sawdust, were continued for eight months, in the year 1818, and the results were the following, four months being employed for using each, under similar conditions:—It appears, first, that the total saving of powder was fully one-third; second, that in the four months during which the mixture was used, although five more men on the average were employed daily, yet only 25 cwt. 37 lbs. were required, instead of 30 cwt. 23 lbs., Silesian measure, leaving a balance in favour of the mixture of nearly 5 cwt.; and, thirdly, that in these months more comparative labour was performed.

Similar experiments were also made at Dortmund, in Westphalia, and were attended with a satisfactory result. It was found that six pounds of mixed powder were equal to eight pounds unmixed, per fathom. It was even found by experiments conducted above ground, that a charge of half powder and half sawdust, well mixed, produced the same effect as powder alone in the sandstone. Counter trials were made of charges as above, and others with the half charge of powder alone, from which it appeared, that although each charge contained the same quantity of powder, yet those without the sawdust were almost entirely without effect. The sawdust from beech and poplar had the same power as that of fir; and it appeared that iron and copper filings answer the same purpose, which proves that the favourable effect produced can be ascribed as little to the looseness of the material mixed with the powder, as to a chemical dissolution of the sawdust; nor can the evolution of gases in the mixture be the cause of it.

The most powerful mixture was proved to consist, not of granulated powder, but of simple powder of purified saltpetre, flower of sulphur, and purified charcoal in the same proportion as the French fowling powder, mixed with beech sawdust. Experiments have been successfully made in other mining districts, as Essen-Werden and the Mark; and measures are being adopted in the mining districts of Westphalia to ascertain with accuracy the exact saving which can be effected.—*Zeitschrift der Mineralogie*, 1829, p. 400.

Mining Journal.

Birmingham Railway.

Great gloom has been thrown over the share market, by the announcement that this company will want two millions more money than the estimate. Was not this the company that some year or two back were, week after week, and month after month, trumpeting forth that such a contract had been made which exactly agreed with the engineer's estimate; that a second was finished some few thousands less than the engineer computed, a third still less, &c. &c.? If all this was true then, how comes it about now, that the whole capital, instead of being quite enough, or too much, is found to be no less than eighty per cent. too little, that is little more than half what it ought to be? We think we have a prospectus in which are these emphatical words, "The cost will be 2,500,000*l.*, and the sufficiency of this estimate has been ascertained by the Committee of the House of Commons, upon the testimony of Mr. Stephenson, supported by two other eminent engineers." What answer can be given to this?

But to be serious, the allowance for contingencies was in this estimate, no less than 294,648*l.* Thus, then, stands the account—

Engineer's estimate	. .	£2,205,352
Do's error	. .	2,294,648
	Total cost	£4,500,000

But it may be 5,000,000*l.*, making the trifling error of two and three quarter millions on two and a quarter!!!

Would it not now be better to abandon this line, with its nine tunnels, and take up Sir John Rennie's, which has only one, and which would cost about one-fourth of the amount, or little more than the second additional sum now wanted. If, as stated in the report, they have spent only 2,280,000*l.*, and have had in cash unpaid shares, and money already borrowed 3,400,000*l.* they will have about enough to make, as we understand, Sir John Rennie's new line, without borrowing this other million. The tunnels might be converted into dark holes for the punishment of culprits, or would make very excellent gipsy retreats, cow-sheds, pig-sties, &c. &c. We recommend the directors and shareholders to take this into their serious consideration, and they will, no doubt, make us a handsome compliment for the suggestion.

Is it true, as whispered about, that a great portion of this new sum wanted has been spent in buying off opposition, &c.?

Why, this line will cost upwards of 40,000*l.* a mile! or more than double the average cost. Surely this is a second edition of the Liverpool and Manchester Railway, which, if we recollect right, Mr. Stephenson undertook to make for about 500,000*l.*, but which, in reality has, we believe, cost near three times that amount! For the shareholders' sakes, and for the sake of burying in oblivion Mr. Stephenson's sad, sad blunder, we hope the success of the one may equal that of the other.—*Railway Magazine.*

Ibid.

Mechanics' Register.

Wood Joists in English Railways.

The substitution of the continuous wood-joist, bedded in concrete, to support the rail, instead of iron chairs and stone bases at intervals only, is about to take place for the first time on an extensive scale in England. The directors of the Great Western Railway, have recently made their first contract for 13,000 loads of timber for this purpose, the whole of which is probably laid down by this time.—*Corr.*

Mag. Pop. Sci.

Soda Bread.

A correspondent of the *Newry Telegraph* gives the following receipt for making "soda bread," stating that "there is no bread to be had equal to it for invigorating the body, promoting digestion, strengthening the stomach, and improving the state of the bowels." He says, "put a pound and a half of good wheaten meal into a large bowl, mix with it two teaspoonfuls of finely-powdered salt, then take a large teaspoonful of super-carbonate of soda, dissolve it in half a teacupful of cold water, and add it to the meal; rub up all intimately together, then pour into the bowl as much very sour buttermilk as will make the whole into soft dough (it should be as soft as could possibly be handled, and the softer the better,) form it into a cake of about an inch thickness, and put it into a flat Dutch oven or frying-pan, with some metallic cover, such as an oven-lid or griddle, apply a moderate heat underneath for twenty minutes, then lay some clear live coals upon the lid, and keep it so for half an hour longer (the under heat being allowed to fall off gradually for the last fifteen minutes,) taking off the cover occasionally to see that it does not burn. This, he concludes, when somewhat cooled and moderately buttered, is as wholesome as ever entered man's stomach. Wm. Clacker, Esq., of Gosford, has ordered a sample of the bread to be prepared, and a quantity of the meal to be kept for sale at the Markethill Temperance Soup and Coffee Rooms.

Farm. Mag.

Amber.

The beautiful amber which is found on the eastern shores of England, and on the coasts of Prussia and Sicily, and which is supposed to be fossil resin, is derived from beds of lignite in the tertiary strata. Fragments of fossil gum were found in digging the tunnel through the London clay at Highgate, near London.

Mining Journal.

Proportion of Crime to Population.

According to Colonel Forsell, who has recently published a valuable work on the statistics of Sweden, the offenders against the laws in different countries bear the following proportions to the entire population:—

In England as one to . .	740
Wales, . .	2,320
Ireland, . . .	490
Scotland, . .	1,130
Denmark, . .	1,700
Sweden, . .	1,500
New South Wales, . .	22
The United States, .	3,500

Farm. Mag.

To make Nectar. (Vin des Dieux.)

Slice very thinly, a quantity of apples, and an equal number of lemons. Cover the bottom of a porcelain dish with a layer of the apples, then a layer of lemon, then a third of powdered sugar and so on till the vessel is full enough. Pour in wine until the upper layer of lemons is well overflowed, put on a cover and let it infuse five or six hours. Press out the liquor through a cloth and filter it.

Jour. des Conn. Us.

LUNAR OCCULTATIONS FOR PHILADELPHIA, AUGUST 1837.

Angles reckoned to the right or westward round the circle, as seen in an inverting telescope. ☞ For direct vision add 180° ☜

Day.	H'r.	Min.	Phase	Star's name.	Mag.	from Moon's North point.	from Moon's Vertex.
5	8	8		N. ap. ☽ and ν Virginis ☽ S 1.′3			
10	10	17	Im.	L Scorpii	1,	16	52
10	10	48	Em.			326	366
14	11	11	Im.	m Capricorni	6,	67	67
14	12	1	Em.			344	354
16	12	13	Im.	K Aquarii,	6,	195	183
16	12	39	Em.			235	229

Meteorological Observations for April, 1837.

Moon.	Days.	Therm.		Barometer.		Wind.		Water fallen in rain.	State of the weather, and Remarks.
		Sun rise.	2 P.M.	Sun rise.	2 P.M.	Direction.	Force.		
				Inches	Inches			Inches.	
	1	40°	44°	29.40	29.56	W.	Blustering		Cloudy—flying clouds.
	2	32	44	85	86	W.	do.		Clear—clear.
	3	38	56	55	55	SW.	Moderate.		Clear—clear.
	4	34	33	75	75	NW.	do.	.4	Cloudy—snow—cloudy.
⊗	5	32	42	70	66	NE.	do.		Cloudy—do.
	6	32	55	80	85	W.	do.		Clear—do.—aurora borealis.
	7	36	55	76	65	SE.	do.		Lightly cloudy—do. do.
	8	52	44	16	10	W.	Brisk.	.60	Rain—thunder shower.
	9	44	46	35	35	W.	do.		Cloudy—flying clouds.
	10	33	34	50	55	W.	do.		Flying clouds—clear.
	11	39	60	60	65	W.	Moderate.		Clear—do.
☾	12	40	64	30.00	95	W.	do.		Clear—do.
	13	46	65	29.80	80	W.	do.		Partially cloudy—do. do.
	14	46	58	40	40	W.	do.		Cloudy—flying clouds.
	15	40	60	70	70	W.	do.		Clear—clear.
	16	46	56	44	40	NE.	do.		Cloudy—do.
	17	42	56	50	51	W.	Brisk.		Partially cloudy—do. do.
	18	36	58	70	70	NW.	do.	·10	Rain—cloudy.
	19	39	79	75	73	W.	do.		Partially cloudy—do. do.
☺	20	37	60	80	80	W.	do.		Partially cloudy—do. do.
	21	39	54	75	80	W.	do.		Clear—do.
	22	35	50	80	95	W.	do.		Clear—flying clouds.
	23	37	41	90	85	NE.	Moderate.		Cloudy—rain—snow.
	24	33	43	80	80	NW.	do.	1.00	Snow—flying clouds.
	25	34	52	80	75	W.	Brisk.		Clear—cloudy
	26	35	56	75	75	W.	Moderate		Clear—partially cloudy.
☽	27	40	60	70	75	W.	do.		Lightly cloudy—hazy.
	28	42	68	30.10	30.15	W.	Brisk.		Clear—hazy.
	29	42	73	41	7	SW.	do.		Hazy—do.
	30	52	76	29.85	29.80	SW.	do.		Hazy—do.
	Mean	39.10	53.73	29.72	29.70			1.74	

		Thermometer.	Barometer.	
Maximum height during the month		76. on 30th.	30.41 on	29th.
Minimum	do.	32. on 2d 5th 6th.	29.10 on	8th.
Mean	do.	46.41.	29..71	

JOURNAL
OF THE
FRANKLIN INSTITUTE
OF THE
State of Pennsylvania,
AND
MECHANICS' REGISTER.
DEVOTED TO
Mechanical and Physical Science,
CIVIL ENGINEERING, THE ARTS AND MANUFACTURES,
AND THE RECORDING OF
AMERICAN AND OTHER PATENTED INVENTIONS.

AUGUST, 1837.

Practical and Theoretical Mechanics.

Report of the Committee of the Franklin Institute of the State of Pennsylvania on the Explosions of Steam Boilers, of Experiments made at the request of the Treasury Department of the United States. PART II. *Containing the report of the sub-committee to whom was referred the examination of the strength of the materials employed in the construction of Steam Boilers.*

[CONTINUED FROM PAGE 31.]

Second method of observing elasticities.]

Another method of approximately determining the elasticity of iron as indicated when subjected to different strains, was to measure directly on the specimen under trial the distance between two points, taken as remote from each other as possible, both when under strain and when that strain was removed.

Thus bar No. 49 having been permanently elongated $\frac{3}{10}$ of an inch, in $20\frac{3}{10}$, under a weight of 273 pounds in the scale, gave a recoil of .05 of an inch. Afterwards with a weight of 301 lbs., and when a permanent elongation of .58 inch, in the same original length had taken place, the recoil amounted to $\frac{1}{348}$ of the total length. After that trial 15 pounds in addition were required to break the bar.

On bar 226 the first permanent elongation was found under a weight of 245 pounds. Under a weight of 280 pounds the elongation in 24 inches was .86 inch, and when relieved it was .82, giving a recoil of .04 in 24 inches,$=\frac{1}{600}$. After this last trial 35 pounds additional weight were required to produce the fracture.

On bar 228, we find that the first elongation was taken under a weight of 232 pounds. With 238 pounds it had become .146 inch on a length of 24; but when relieved the recoil was .046 inch, equal to $\frac{1}{525}$ of the length. Twenty-eight pounds were afterwards required to be added to break the bar.

On bar 230 the elongation took place under 196 pounds. With 317 pounds the recoil on a part originally 24 inches long was .05 inch, equal to $\frac{1}{450}$ of the whole original length, and 13 pounds more were required to produce the fracture.

Experiments and remarks on this subject will be found in Tables XXXVII., XL., LV., LIX., &c.

TABLE XCII.—*Synopsis of the elasticity of 56 different bars of iron of given lengths, under certain weights, and also the breaking weights of the same bars as found immediately subsequent to the*

No. of the bar and direction of the slit.	Length under trial.	Weight in the scale.	Elasticity observ. corrected for the machin.	No. of the bar & direc. of the slit.	Length under trial.	Weight in the scale.	Elasti. observed correct. for the machine.	No. of bar & direction of the slit.	Length under trial.	Weight in the scale.	Elastic. observed correct. for the machine.
2.	23.55	280	15′	39. C.	23.5	224	20′			452	Broke.
L'gth.		336	16			314	31	58. C.	22.35	448	26′
		392	19			317	Broke.			479	Broke.
		448	25	41. L.	23.35	224	45	58. C.	20.7	479	57
		474	21.5			282	27.5			496	Broke.
		479	Broke.			292	Broke.	59. L.	23.5	112	12.5
4. L.	13.2	224	35	42. L.	23.8	224	27			168	20
		336	38			336	32			224	27
		448	16			399	28.5			242	27
		460	Broke.			427	31			258	Broke.
8.	21.2	280	20			443	Broke.	61. L.	23.6	224	40
Cross.		336	22	42. L.	22.45	399	28.5			280	42
		392	15			427	31			326	38
		430	15			452	Broke.			332	36
		443	Broke.	42. L.	18.8	399	28.5			336	Broke.
17. Cr.	23.9	259	23			462	22	68. L.	24.	224	32
		280	22			462	Broke.			336	31
		294	14	44. L.	23.7	224	25			392	28
		326	17.5			336	25			414	30
		336	21			448	23			441	32
		350	24			476	36			450	Broke.
		364	31.5			496	34	68. L.	20.8	224	32
		371	27			503	Broke.			448	20
		374	Broke.	46. L.	24.4	224	29			455	Broke.
18. Cr.	Unc.	392	32			280	29	70. L.	24.2	112	19.5
		403	37.5			297	28.5			224	16
		412	39			314	Broke.			280	19
		415	26	48. L.	24.5	224	16			336	23
		415	Broke.			336	18			364	26
21. L.	17.4	280	25			425	21.5			377	Broke.
		321	26.5			444	30	64. C.	24.2	112	38
		336	28			464	27			224	55
		356	Broke.			478	31			280	51.5
23. L.	24.3	280	21			487	28			301	52.5
		364	31			503	Broke.			329	46
		393	Broke.	51. C.	24.	224	53			335	Broke.
23. L.	21.1	280	29			336	53	65. C.	24.25	224	19
		364	22			448	27			280	18
		413	57			464	32			336	27
		426	Broke.			472	Broke.			369	Broke.
25. L.	24.1	224	39	53. C.	24.	224	37	71. C.	24.1	224	20
		361	28.5			336	36			336	27
		370	Broke.			448	21			357	Broke.
27. L.	24.2	224	35			464	22	71. C.	23.1	336	28
		336	38			478	Broke.			392	26
		392	53	53.	21.	494	25			414	Broke.
		403	Broke.			500	Broke.	73. C.	23.95	112	5.5
32. Cr.	23.	112	10.5	56. C.	23.7	224	07			224	11
		168	10			336	07			280	18
		224	12			448	21			308	19.5
		280	13.5			471	Broke.			322	33
		336	16	56. C.	20.3	448	25			336	24
		392	17			495	Broke.			342	Broke.
		410	Broke.	58. C.	23.7	224	49	85. L.	24.5	112	11.5
37. L.	22.	112	23			336	54			224	15
		162	43			392	50			336	33
		171	Broke.			448	44			392	38

[TABLE XCII. continued] *time of taking the elasticity. The direction of the slitting is indicated by the letters in the left hand column.*

No. of the bar and direction of the slit.	Length under trial.	Weight in the scale.	Elastic. observed correct. for the machine.	No. of the bar and direction of the slit.	Length under trial.	Weight in the scale.	Elastic. observed correct. for the machine.	No. of the bar and direction of the slit.	Length under trial.	Weight in the scale.	Elastic. observed correct. for the machine.
		420	36′	101. L.	25.1	224	35′			420	51′
		448	24			336	41			438	49
		462	Broke.			403	Broke.			448	45
85. L.	22.1	224	15	103. C.	23.8	224	38			462	46.5
		336	23			280	34			468	Broke.
		392	24			308	43.5	174. D.	30.2	219	45.5
		462	28			321	Broke.			227	50.5
		481	Broke.	103. C.	9.6	168	7			235	Broke.
87. L.	24.5	224	21			361	Broke.	174. D.	27.7	231	65
		336	16	105. L.	24.2	224	63			235	Broke.
		392	25			294	64	174. D.	23.9	231	41
		441	25			320	63			245	44
		464	Broke.			322	Broke.			246	Broke.
75. L.	24.6	257	8	130. C.	16.6	56	8	174. D.	22.1	245	39
		262	Broke.			112	8.5			246	Broke.
75. L.	21.4	168	10			168	9	148. C.	unc.	212	29
		267	Broke.			224	11			336	33
78. L.	24.4	112	14.5			280	13.5			392	42
		224	17.5			336	12			448	45
		280	21			436	16			463	Broke.
		280	Broke.			466	12	148. C.	20.6	463	37
78. L.	Unc.	224	54			485	16			476	Broke.
		293	Broke.			506	24	151. C.	30.5	224	30
83. C.	24.	224	34			529	Broke.			336	33
		273	Broke.	137. Diagonal.	4.	490	44			448	40
84. C.	23.8	168	50			498	Broke.			476	Broke.
		224	52	142. L.	30.4	112	8.5	151. C.	29.8	476	36
		275	54			224	14			489	Broke.
		289	Broke.			280	15	167. C.	30.4	224	38
84. C.	9.	224	33			336	27			266	36
		317	24			364	26.5			284	42
		319	Broke.			380	32			292	Broke.
84. C.	15.2	280	53			398	31.5	167. C.	19.33	306	29
		294	Broke.			415	Broke.			307	Broke.
91. C.	24.	112	50.5	143. L.	30.6	224	30	160. L.	30.2	224	82
		224	76			280	33			245	Broke.
		280	60			336	48	160. L.	22.25	252	64
		347	34			370	53			257	Broke.
		364	20			392	52	162. L.	30.2	168	60
		392	Broke.			435	44			224	65
94. L.	24.07	224	37			440	47			252	58
		280	45			440	Broke.			256	Broke.
		336	38	143. L.	29.3	464	41	162. L.	27.6	262	72
		392	36			478	Broke.			262	Broke.
		448	25	154. D.	30.4	224	30	181. Steel bar.		112	20.5
		483	25			336	30			224	30
		511	Broke.			392	38			336	28
95. C.	24.1	224	34			420	42			385	36.5
		336	26			427	Broke.			406	40.5
		382	Broke.	154. D.	23.08	392	21			415	39.5
99. C.	24.3	224	20			438	Broke.			422	36.5
		336	36	157. D.	30.35	224	27			429	37.5
		378	42			336	50			441	Broke.
		378	Broke.			392	50				

TABLE XCIII.

Comparative view exhibiting the areas of section at the points of fracture in 151 experiments on 67 different specimens of iron, embracing

No. of the comparison.	Number of the specimen referred to.	No. of experiments on the respective specimens.	Area, after fracture, of strips cut lengthwise, compared with their original area as unity.	Area of section of fracture of specimens cut transversely, their original area being 1.	Mean area of length strips in each kind of iron.	Mean area of cross strips in each kind of iron.
1	2	2	.887			
2	4	2	.829			
3	6	2		.904		
4	8	1		.895	.858	.899
5	9	1	.876			
6	11	2	.771			
7	21	2	.909			
8	23	3	.876			
9	13	2		.860		
10	15	1		.867		
11	18	1		.964	.858	.897
12	25	6	.824			
13	27	1	.936			
14	35	1	.909			
15	37	1	.949			
16	32	1		.840		
17	39	1		.934		
18	41	1		.927	.904	.900
19	42	3	.923			
20	43	1	.924			
21	44	2	.914			
22	56	2		.920		
23	58	4		.883	.920	.901
24	46	2	.790			
25	48	2	.761			
26	51	2		.838		
27	53	2		.865	.775	.851
28	59	1	.890			
29	61	1	.891			
30	64	1		.967		
31	65	1		.940	.890	.953
32	68	2	.833			
33	70	1	.858			

Number of the comparison.	Number of the specimen.	Number of experiments on the respective specimens.	Area after fracture of strips cut lengthwise, compared with their original area as unity.	Area after fracture of specimens cut transversely.	Area after fracture of specimens cut diagonally.
34	71	4		.781	
35	73	1		.830	
36	85	3	.889		
37	87	2	.800		
38	75	1	.869		
39	78	1	.840		
40	81	1		.978	
41	83	3		.901	
42	84	3		.859	
43	94	4	.760		
44	95	5		.872	
45	99	1		.947	
46	103	2		.810	
47	107	3		.816	
48	101	2	.849		
49	105	1	.895		
50	125	2	.502		
51	130	1		.766	
52	133	5		.587	
53	135	5			.512
54	137	3			.544
55	142	1	.941		
56	143	3	.780		
57	160	5	.772		
58	162	2	.735		
59	164	5	.457		
60	148	2		.885	
61	151	5		.847	
62	167	2		.729	
63	169	6		.835	
64	154	3			.877
65	157	1			.842
66	171	1			.412
67	174	4			.805

TABLE XCIII.

11 kinds of metal, distinguishing those specimens which were cut lengthwise from those which were cut crosswise and diagonally from the sheet.

Mean area of longitudinal specimens.	Mean area of transverse specimens.	Mean area of diagonal specimens.	REMARKS.
·845	.805		From this table it appears that the areas of fracture in strips cut across the direction of the rolling, are greater than in those cut longitudinally. The difference in this respect on the 12 different kinds, averages 6 per cent. of the area of the fractures in the longitudinal strips,—a difference corresponding very nearly with the difference in *strength* of longitudinal and transverse strips as contained in Tables XCV. and XCVI. It also appears that of the 12 different sorts of iron compared. 8 exhibit a diminution of area in the *length* strips, greater than in those cut across the direction of the rolling, and 4 show a small balance in the opposite direction. The mean difference of the 8 sets first mentioned, is .861—.778=.083 of the original area, or .107 of .778, the remaining area in the case of the length strips. The mean of all the 67 comparisons in all directions exhibits the diminution of area from 1.000 to .835, or a "constriction" of .165=1-6 the original size, and of course a correspondent increase of length at the parts in the immediate vicinity of the fractures.
.849	.912		
.760	.872		
.872	.857		
.502	.679	.523	
.737	.824	.859	

Diminution of area at the moment of fracture.

With a view to determine within certain limits the extensibility of iron when subjected to strain, and to compare the same in specimens cut across the sheet with that of longitudinal strips, a considerable number of measurements were taken at the sections of fracture after the experiment, and from these the above table of results is exhibited, indicating the mean result of the trials on each bar and the ratio of the remaining area to the original area taken as unity.

In taking these measurements some little uncertainty is to be admitted, owing to the fracture taking occasionally a diagonal direction, but as the two fragments afforded the means of obtaining corrections, the error from this cause cannot have, at most, exceeded a few thousandths of an inch.

By referring to the table of permanent elongations as taken on the whole bar, (Table XCIV.), it will be seen that the greatest extension observed on an entire bar, previous to the first fracture, was 3 inches in 24 or $\frac{1}{8}$ of the total length. This was on a bar reduced to a uniform size. But as no bar of iron, of any considerable length, is of uniform strength throughout, we are not to expect in any case an extension in length *of the whole bar* equal to the diminution of area at the point of fracture.

One remark worthy of particular attention, in connection with our subject, is, that at elevated temperatures, before the diminution of strength has begun to be felt, the diminution of area or *constriction* of iron is often much less, than when the trial is made on the cold metal. This is particularly exemplified in bar No. 164, in which two experiments at 577°, gave a tenacity 14 per cent. greater than five others, made at from 75° to 80°, while the constriction was less in the hot trials than in the cold, in the proportion of .338 to .447, or about *one-third.* In this and similar cases the fractures at high temperatures were observed to take place suddenly, and the surfaces of fracture to present appearances altogether different from those found in cases where the same bar was broken cold. This peculiarity consisted in a smooth section, directly across the breadth of the filed portion in which they took place, but uniformly inclined to the flat face of the bar, in an angle of about 45 degrees, and presenting therefore a bevel, like the cutting edge of a common mortising chisel.

In a few instances, particularly in experiments on bars as they came from the shears, the fracture was compound, the strengths at two neighboring sections being so exactly equal as to separate simultaneously, at the distance of half an inch or an inch from each other.

Bars 228 and 230, the former of which was cut crosswise, and the latter lengthwise of the sheet, and both broken up at ordinary temperatures, indicated a marked difference in the nature of the surfaces of fracture. The former presents, in nearly all instances, irregular and jagged surfaces inclining to compound fracture, or displays rough sections perpendicular both to the edges and faces of the bar. The latter is oblique and fibrous.

The amount of constriction in strips cut across the direction of rolling is, on an average, about 6 per cent. less than in those cut longitudinally.

A careful comparison of the breadths and thicknesses before and after fracture would show that the diminution in thickness follows a more rapid rate than that in breadth, whether the iron be hammered or rolled, and whether in the latter case, it be cut lengthwise or crosswise of the sheet.

The amount of constriction observed, viz: 16½ per cent., is rather less than that obtained by M. Martin,* who operated chiefly on rolled bars or bolts of considerable magnitude, and found on an average of 35 comparisons $18\frac{6}{10}$ per cent. of elongation.

The above table will show that the difference in different kinds of metal in respect to the diminution of *area* of fracture is very marked, sometimes exceeding 54 per cent. of the whole original area; while at others it scarcely exceeds 5 per cent.

The difference in the extensibility of iron in the longitudinal and the transverse directions of the sheet is liable to manifest itself in practice, when a portion of a boiler becomes locally so overheated as to bulge out in a particular spot. It will then be seen that an elongated protuberance is exhibited, having the greater axis in the direction across the sheet, and the less lying in the course of the rolling.

The piece taken from an old boiler which had been burst in consequence of the gradual overheating of a portion over the fire where sediment had collected, and which will be referred to in another part of this report, exemplifies the kind of action above described. The extent of the swelling in the direction of the length of the sheet was $10\frac{1}{2}$ inches, which, measuring over the summit of the bulge, had become $12\frac{1}{4}$ inches; or the increase of distance over the surface of the metal was .167 of the original extent. In the transverse direction of the rolling, the original length of the swollen part was $20\frac{1}{2}$ inches, and the line applied over the summit measured 23 inches, or the increase in this direction was .122 of the original length. Hence the extensions of this specimen of iron in the two directions are to each other as 167 to 122.

Some idea may be formed of the extent of constriction in *breadth*, both of iron and copper bars under various temperatures, by inspecting Plate II. *m* and *m'* are two portions of bar No. 164, Table XLVIII., which was prepared by filing notches on its sides. The portion *m*, of which the breadth before trial was .747 inch in the deepest part of the filed section and .132 inch thick, was broken at a temperature of 576°. Strength, 66336 pounds per square inch. The breadth after fracture was .674 inch, and the thickness .102. The surface clean, smooth and bevelled in an angle of about 45°. The diminution of area is consequently $.098604 - .068781 = .029823$ square inch. *m'* represents a portion broken in experiment 15 of the same table, at a temperature of 87.5°, giving a strength of 56503 pounds per square inch, and a section of fracture measuring .601 in breadth and .07 in thickness;—whereas the original breadth had been .731 and thickness .138; so that the constriction was here $.100878 - .042070 = .058808$ square inch,—almost exactly double as much as when broken at 576°. Experiment 17 on the same bar affords another illustration of the effect of a moderately elevated temperature in preventing constriction.

The original breadth of the bar in which these notches were filed was 1.117 inch. Hence the notches in *m* were each $\frac{1.117 - .747}{2} = .185$ inch deep, and those in *m'* each about .193 inch, in both cases quite sufficient to preclude the supposition of any weakening effect of the shears, within the part left after filing.

n and *n'* are portions of the bar of iron, No. 224 C, an account of which is given in Table LXXII. The breadth before trial of this bar, which was reduced to uniform size by filing, is indicated by the dotted lines outside of the plain ones, while the inside dotted lines mark the breadths after fracture. Both portions were broken at low temperatures. n' in experiment 5 of that table, at a temperature of 80°, exhibited a strength of 62472 pounds per square inch, a breadth of section=.572 inch, and a thickness of .159, consequently giving an area of section after fracture of .090948 square inch, whereas the area before trial was .182945, and the constriction .091967, a trifle more than 50 per cent. of the original section. The fracture on the portion n was made at the 8th experiment of the same table when the temperature was 71°, and within one inch of the point broken in the trial just referred to. But previously to this fracture, the specimen had been submitted without access of air to a bright welding heat, so as perfectly to anneal the iron without oxidizing it. The strength was then found to be only 36052 pounds per square inch, reckoned on the original section at that part of the bar, the breadth being .457 inch, the thickness .118, and the area .053926; while the original breadth had been .757, original thickness, .24125, and area .182626, which had been reduced before the annealing to .150664. Hence the constriction after annealing was .182626—.053926=.128700, or 70.4 per cent. of the original cross section of the bar.

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* See Annales des Mines, 3d series, Vol. V. 1st part.

o and o' represent specimens of copper bar, No. 7 table XXII., to which we have already referred in speaking of the extensibility of that metal.

Forces producing permanent elongations of iron.

In connexion with the subjects of tenacity and elasticity, it has generally been deemed important to pay some attention to the relation between the forces which will break, and those which will elongate the specimens to a sensible degree, rendering them incapable of returning to their original dimensions. The committee have not been unmindful of this subject, and the following table will exhibit the most important of these observations, which have, during the course of our experiments, been made to bear directly on this point. An inspection will show that the first permanent elongation may take place under forces varying according to the character of the materials. Those kinds which possess the greatest extensibility begin in general earliest to manifest this property, in yielding *permanently* to low degrees of force. This remark is exemplified by a comparison between Nos. 226 and 219 A., the latter of which showed an extensibility before fracture of 2.5 inches in 24, or about $\frac{2}{19}$ of its whole length, and began to extend with $41\frac{2}{3}$ per cent. of the breaking weight, while the former was extended $\frac{1}{16}$ of its length, or $1\frac{1}{2}$ inch in 24, and required 76 per cent. of the breaking weight to cause the first elongation. The *extremes* lie between .416 and .872 of the ultimate strength; and the mean of 13 comparisons is .641, conforming nearly with the results obtained by former experimenters.

The eighth and ninth columns of the following table show the total elongation at the moment of fracture. This must necessarily be different in different bars, as well on account of the diversity in their constitution, as of the unequal degrees of *uniformity* in size and structure in different parts of the same bar.

TABLE XCIV.

Comparative table exhibiting the amount and relations of the weights required to produce the first permanent elongation in different bars of iron, and the weights required for the first fracture of each bar, also the amount of permanent elongation of the specimen, and the ratio which that elongation bears to the entire length before trial.

No. of the Comparison.	Mark of the bar affording the comparison.	Original length of the part measured.	Strength in lbs. per square inch, exhibited by the first fracture.	No. of pounds in the scale at the first frac.	Wt. in the scale at 1st permanent elongation.	Ratio of the *elongating* to the *breaking weight.*	Elongation in inches, at the time of fracture.	Pt. by which the original l'gth was increased.	REMARKS.
1	49	20.3	57565	316	245	.775	1.42	$\frac{1}{14}$	When strained with 273 lbs. and then relieved, the recoil of the part within the marks originally 20.3 in. apart, was .05 of an inch; with 301 lbs. in the scale it was .06, and with 306 lbs. it was .04. The mean of these 3 trials of the recoil, shows that it amounted to $\frac{1}{406}$ of the original length.
2	149	24.	52778	334	224	.670			
3	150	24.	59397	346	196	.566	1.5	$\frac{1}{16}$	
4	191	15.	47991	326	210	.644	1.42	$\frac{1}{11}$	
5	219 A.	24.	52257	267	112	.416	2.5	$\frac{1}{9.6}$	
6	220 B.	24.	58642	336	184	.547	3. +	$\frac{1}{8}$	Under 289 lbs. the 24 inches had become 27, and the bar had extended very uniformly.
7	223 A.	24.	43766	269	168	.622	1. +	$\frac{1}{24}$	
8	223 B.	22.	41555	262			2.55	$\frac{1}{8.6}$	The first permanent elongation of this bar was not observed, but under a weight of 245 lbs. in the scale the elongation was .7 inch.
9	226	24.	49053	315	240	.761	1.5	$\frac{1}{16}$	
10	228	24.	40643	266	232	.872			Under a weight of 238 lbs. in the scale, the elongation in 24 inches was .146 inch; but when relieved the recoil was .046, showing the permanent elongation to be .1 inch, and the ratio of the recoil to the total length $\frac{1}{521}$.
11	229	12.	46473	301	210	.697			
12	230	26.	49368	330	196	.594	2.36	$\frac{1}{11}$	
13	231	22.	68513	465	308	.662			
14	232	24.	57039	376	196	.508			
			Mean of 13			.641			
			Maximum Minimum			.872 .416			

It is not only desirable to mark the force which will produce the first permanent elongation of iron, but also to ascertain the successive elongations under different weights, since in the case of the steam-boiler it may be necessary to know what degrees of distortion in its form would result from the various forces which might be applied to it. It is true that in producing these successive elongations *time* may enter as an element into the result; but the experiments of the committee on this subject, were generally conducted with such deliberation as to preclude the supposition that a longer continuance would have materially altered the effects observed. In determining this question, recourse was had to two methods; first, that of direct measurement of the lengths after certain elongating forces had been applied, and secondly, that of gauging the cross sections at numerous equi-distant points along the bar, the results of which showed the irregularity of extension as well as its actual amount in the vicinity of each section so gauged.

Table XLVIII. affords an example of the latter kind of trials, in which the bar was gauged each time, after eight different experiments.

Tables XXXVIII., XXXIX., XLII., LXXXII., LXXXIII., LXXXIV. and LXXXVII., will also be found to contain accounts of similar measurements of cross sections.

Experiments and observations on the progressive extensions *in length* of bars of iron, will be found in Tables XXXVII., XXXVIII., LXV., LXXXII. and LXXXIV.

Strength of iron in different directions of the rolled sheet.

In obtaining specimens for these experiments, care was generally taken to have them cut in different directions of the rolling, longitudinally and transversely, and in some cases *diagonally*, to that direction. The tables will be found to indicate the direction of slitting in each case, and the comparison contained in Tables XCV. and XCVI. is given to show what information the inquiry has elicited.

The comparison is made principally on the *minimum* strength of each bar, being that which can alone be relied on in practice; for if the strength of the weakest point in a boiler be overcome, it is obviously unimportant to know that other parts had a greater strength. In one case, however, two bars, one cut across the direction of rolling and the other longitudinally, were, after being reduced to uniform size, broken up cold, with a view to this question. The result shewed that the length-strip was $7\frac{1}{10}$ per cent. stronger than the one cut crosswise, considering the tenacity of the latter equal to 100. Of the other sets, embracing about 40 strips cut in each direction, it appears that some kinds of boiler iron manifest much greater inequality in the two directions than others. It is in certain cases not much over one per cent., and in others exceeds twenty, and as a mean of the whole series it may be stated to amount to six per cent. of the strength of the cross-cut bars. The number of trials on those cut diagonally is not perhaps sufficiently great to warrant a general deduction, but so far as they go, they certainly indicate that the strength in this direction is less than in either of the others.

Had we compared the mean instead of the least strength of bars as given in the table, the result would not have differed materially in regard to the relative strength in the respective directions.

For this purpose the boiler-iron manufactured by Messrs. E. H. & P. Ellicott, which was tried in all three of the directions of the sheet, and by all the three modes of preparation of specimens, will be found to give the following results,—viz, 1. When tried at *original sections*, seven experiments on length-sheet specimens gave a mean strength of 55285 lbs. per square inch, the lowest being 44399, and the highest 59307. Fourteen experiments on cross-sheet specimens gave a mean of 53896 lbs., the lowest result being 50212, the highest 58839; and six experiments on strips cut diagonally from the sheet, exhibited a strength of 53850 lbs., of which the lowest was 51134, and the highest 58773.

2. When tried by filing notches on the edges of the strips to remove all weakening effect of the shears, the *length-sheet* bars gave, at fourteen fractures, a mean strength of 63946, varying between 56346 and 78000 lbs. per square inch. The cross-sheet specimens tried after this mode of preparation, exhibited, at three trials, a mean strength of 60236 lbs., varying between 55222 and 65143; and the *diagonal* strips, at four trials, gave a mean result of 53925, the greatest difference being between 51428 and 56632.

3. Of strips reduced to uniform size by filing, four comparable experiments on those cut lengthwise of the sheet, gave a mean strength of 63947, of which the highest was 67378 and the lowest 60594.

Cross-sheet specimens tried after the same preparation, exhibited, at thirty-three fractures, a mean of 50176, of which the highest was 65785 and the lowest 52778. No bar cut diagonally was reduced to uniform size.

From the foregoing statements it appears that by filing in notches and filing to uniformity, we obtained results 63946 and 63947 for the strength of strips cut lengthwise, differing from each other but a single pound to the square inch, and that by these two modes of preparation the cross-sheet specimens gave respectively 60236 and 60176, differing by only 60 lbs. to the square inch. This seems to prove that by both methods of preparing the specimens the accidental weakening effect of slitting had been removed by separating all that portion of the metal on which it had been exerted. Hence we may infer that the differences between length-sheet and cross-sheet specimens are really and truly ascribable to a difference of texture in the two directions, which will be seen to amount, in the case of filing in notches, to 6.15 per cent., and in that of filing to uniformity, to 6.26 per cent. of the strength of the cross-sheet specimens.

The single exception to the law that the greater strength is given in the longitudinal direction of the rolling, will be found explained in the remark appended to table XCV.

TABLE XCV.

Comparative view of the strength of specimens of ten different sorts of boiler and one of bar iron, in the longitudinal, transverse, and diagonal direction of the rolling, as deduced from the least

No. of the specimen referred to.	Strength in the longitudinal direction.	Strength in the transverse direction.	Specific gravity.
2	58977		7.7169
3	53828		7.7169
4	4714		7.7169
6		52280	7.7169
8		50103	7.7169
Mn.	53324	51191	
9	57952		7.7874
10	64133		7.7874
11	42000		7.7874
13		50488	7.7874
14		52542	7.7874
15		50166	7.7874
16		50039	7.7874
17		44249	7.7700
18		50218	7.7700
19		49125	7.7764
21	38618		7.7700
22	47491		7.7700
23	42798		7.7700
Mn.	48832	49546	
25	43921		7.7640
27	55636		7.7640
30		44703	7.7640
32		52197	7.7640
35	43237		7.7954
37	46155		7.7954
39		40595	7.7954
41		37713	7.7954
Mn.	47237	43802	
42	51653	Puddled.	7.6820
43	44102	do.	7.6820
44	53836	do.	7.6820
46	59262	H'd pla.	7.6785
48	59418	do.	7.6785
49	57565	do.	7.6785
51	H'd pl	59656	7.7567

No. of the specimen referred to.	Strength in the longitudinal direction.	Strength in the transverse direction.	Specific gravity.
53	H'd pla.	56062	7.7567
56	Puddled.	57926	7.6511
58	do.	50570	7.6511
59	48308	Puddled.	7.6013
60	58684	do.	7.6013
61	52869	do.	7.6013
62	57612	do.	7.6013
64	Puddled.	45392	7.6511
65	do.	51255	7.6511
68	57929	H'd pla.	7.7900
70	47638	do.	7.7900
71	H'd pla.	54634	7.7900
73	do.	52657	7.7900
74	do.	49351	7.7910
Mn.	54074	53049	
75	60408		7.7580
78	41734		7.7580
81		42903	7.7580
83		42162	7.7580
84		42696	7.7580
85	48694		7.7580
86	58969		7.7580
87	50249		7.7580
88	49508		7.7922
90		45060	7.7580
91		42365	7.7580
Mn.	51760	43037	
94		53811	
95		45471	
99		49548	
101	49258		
103		41319	
105		44591	
107		55461	
Mn.	49258	48366	

No. of the specimen referred to.	Strength in the longitudinal direction.
125	57182
130	Tilted.
133	do.
135	do.
137	do.
Mn.	57182
142	44399
143	53135
146	60594
148	
149	
150	
151	
152	
154	
157	
160	
162	
164	56346
167	56682
169	54361
171	
174	
Mn.	54253
200	
201	
206	44149
207	48120
208	52175
Mn.	48148
226	
227	
228	
229	
230	49368
Mn.	49368

TABLE XCV.

strength of each specimen, and the average minimum of each sort of iron, in each direction in which it was tried, together with the specific gravity of the several bars.

Strength in the transverse direction.	Strength in the diagonal direction.	Specific gravity.	REMARKS.
Tilted.			The only set of these experiments in which the bars cut crosswise of the sheet appear to be stronger than those cut lengthwise, is that from 9 to 23,—of which 3 of the length strips were from the same specimen,—a specimen in which a large flaky portion was developed by the trial. This set is therefore not to be taken into the account in computing the relative strength of the two classes of bars. The specimens from 42 to 74, were partly puddled iron, and partly Juniata blooms, hammered and rolled into plate. The length and the cross-sheet specimens of these two kinds must be compared separately as shown in the table (XCVI.) of general results. All the experiments on No. 228 (cross,) and 230 (length,) were made at ordinary temperatures with a view to this comparison.
57789			
53176			
	47738		
	50358		
55882	49048		
		7.7390	
52468			
52228		7.7774	
56869		7.7774	
53811			
56073		7.7774	
	51134		
	52102		
53862			
50212			
	55612		
	51425		
53646	52568		
40163			
46970			
43566			
49053		7.7428	
53699		7.6675	
40643		7.6675	
46473		7.7428	
		7.6675	
47467			

TABLE XCVI.

General results of the several comparisons between bars cut in the different directions.

Sets of specimens compared.	No. of experiments on longitudinal bars.	Strength of specimens cut lengthwise.	No. of experiments on transverse bars.	Strength of specimens cut crosswise.	No. of experiments on diagonal bars.	Strength of bars cut diagonally.	Excess of longitudinal, over transverse specimens.	Excess compared with the whole strength of the cross cut bars as unity.
2— 8	3	53324	2	51191			2133	.0416
25— 41	4	47237	4	43802			3435	.0784
75— 91	6	51760	5	43037			8723	.2026
94—107	1	49258	6	48366			892	.0184
125—137	1	57182	2	55482	2	49048	1700	.0306
142—174	6	54253	7	53646	4	52568	607	.0113
200—208	3	48148	2	43566			4582	.1051
226—230	1	49368	4	47467			1901	.0421
Ham'd plate.	5	56366	5	54872			1894	.0347
Puddled iron.	7	52437	4	51411			1026	.0200
Mean of	37	51933	41	49247	6	50808	2689	.0585
Mn. of ex. on 228—230	13	54022	13	50433			3589	.0711

From the above comparison of nearly eighty specimens, from ten different sorts of iron, it appears that the average minimum strength of iron in the direction in which it is rolled, is very nearly 6 per cent. greater than that in the transverse direction.

Specific gravity of Boiler-Iron.

Table XCV. contains in addition to a view of the relative tenacities of strips cut lengthwise and crosswise of the boiler plate, a column exhibiting the specific gravities of the several kinds of iron there compared. In most cases, these were obtained by cutting from the same sheet which furnished the strips, and in a contiguous part, a small sample, expressly intended for a trial of its density. Several bars will consequently be found to have the same specific gravity assigned to them. But we have still no less than nineteen different experiments from which to deduce the general result. From these it appears that the highest density was 7.7922, the lowest 7.6013, and the mean 7.7344; also, that the difference .1909, between the maximum and the minimum is $\frac{1}{40\cdot5}$ part of the mean density, which may accordingly be taken as the limit of variations in this particular.

Seventeen different trials of specific gravity on *bar* iron gave a mean of 7.7254, and the greatest difference between any two results was 7.8319—7.4587=.3832, or $\frac{1}{20\cdot1}$ of the mean density.

Effect of repeated Piling on the tenacity of Plate-Iron.

The effect of repetitions of the process of piling and welding, whether of puddled iron or blooms, is exhibited in table XCVII., in which are contained the results of trials on three specimens of iron, furnished by an establishment in Centre County, Pennsylvania, a region known to afford an ore of superior quality, and which has long supplied wrought iron, inferior to few other kinds of the article known in the American market.

TABLE XCVII.

Experiments on bars No. 242, 243 *and* 244. *Manufactured by Messrs. Valentine & Thomas, near Bellefonte, Centre County, Pa., from metal obtained from* pipe ore, *found* 15 *miles north of that place. No.* 242 *from "refinery bloom", extended into bars three inches wide by one inch thick, piled four high and twice welded. No.* 243 *also from "refinery bloom," twice welded. No.* 244 *manufactured from puddled iron, rolled at the first operation into* 3½ *by* 1 *inch, then cut up and piled* 4 *tiers high, welded again and extended into a billet, then a second welding and rolling into the shape in which it was received.*

No. of the bar.	No. of the experiment.	DATE.	Breadth before trial.	Thickness before trial.	Area of section before trial.	Breaking weight in the scale.	Breaking weight × leverage.	Friction.	Effective strain.	Strength in pounds per square inch.	REMARKS.
242	1	1835. Nov.	.753	.222	.167166	342	10260	513	9747	58308	All the bars had been reduced when received to such a state of uniformity in size as allowed them to be considered as equal throughout and equal to each other. The experiments on 242 prove that the mean str'gth is 59247 lbs., and the irregularity between experiments 1 and 3 is 2047, or a little more than 1-29 of the mean strength.
"	2	"	do.	do.	do.	349	10470	523	9947	59505	
"	3	"	do.	do.	do.	354	10620	531	10089	60355	
"	4	"	do.	do.	do.	345	10350	517	9833	58820	
243	5	Nov.	.753	.222	.167166	340	10200	510	9690	57972	From experiments on this bar it appears that the mean strength was 58787 lbs., the difference between experiments 1 and 5 is 2718 or the irregularity amounts to 1-21.6 part of the mean strength.
"	6	"	do.	do.	do.	340	10200	510	9690	57972	
"	7	"	do.	do.	do.	343	10290	514	9676	58480	
"	8	"	do.	do.	do.	345	10350	517	9833	58820	
"	9	"	do.	do.	do.	356	10680	534	10146	60690	
244	10	Nov.	.753	.222	.167166	332	9960	498	9462	56600	From these experiments it is seen that the mean strength of the puddled bar was 57513 lbs.—the greatest difference, that between experiments 1 and 6, is 2390, or almost exactly 1-24 of the mean strength.
"	11	"	do.	do.	do.	335	10050	502	9548	57115	
"	12	"	do.	do.	do.	335	10050	502	9548	57115	
"	13	"	do.	do.	do.	336	10080	504	9576	57285	
"	14	"	do.	do.	do.	340	10200	510	9690	57972	
"	15	"	do.	do.	do.	346	10380	519	9861	58990	

Results.—1. The specimen which had been made from *refinery bloom* reduced to a bar 3 inches wide by 1 thick, piled 4 high and twice welded, proved the strongest. Not only did it exhibit the high mean result of 59247 pounds to the square inch, but the consistency and uniformity of the metal were such as to show between the highest and the lowest results a difference amounting to only 3.4 per cent. of that mean.

2. The specimen which had undergone two weldings, but had not been drawn into a bar and piled, was inferior to the preceding, giving a mean result of 58787; and the difference between the extremes exceeds 4.6 per cent. of that amount.

3. The specimen manufactured from puddled iron, rolled at the first operation into a bar $3\frac{1}{2}$ inches wide by 1 inch thick, cut and piled 4 tier high and then twice welded and rolled into bars little more than $\frac{2}{10}$ of an inch in thickness, was obviously designed by the manufacturers to correspond in all particulars with No. 1, except that the latter was from the bloomery, and the other (No. 3.) from the puddling furnace. The mean absolute strength of this specimen was decidedly less than that of either of the preceding, being only 57613 pounds, while the variation from uniformity was 4.1 per cent. of its mean strength.

Effects of Piling into the same Plate, Iron of different degrees of fineness.

It is believed to be a practice, not unknown among the manufacturers of boiler iron to combine into the same slab, and subsequently to roll into plate, iron of different qualities. In such cases the want of homogeneousness may manifest itself in a variety of ways. The different kinds may not be equally welded together throughout, and may consequently exhibit flaws at the surfaces of lamellation. They may be very unequally extensible under the same force, and while the outer ply of metal may, in turning a flanch, remain unbroken, the interior one may be greatly reduced in strength. They may be of different specific gravities and mislead the purchaser, who looks only upon the exterior face of the plate, and judges of its value by the good appearance of the latter, and the weight of the whole per square foot under a given thickness.

By a reference to table XXXIII., in experiments on bars 39 and 41, it will be seen that a considerable defect, of the nature above alluded to, was exhibited, and the low results given in those cases, (40600 and 37700 lbs. per square inch) afford conclusive evidence of the bad quality of boiler plate in which such defects of welding between the laminæ exist.

In table XLIX., experiment 16, another example of the same kind occurs, in which a decided difference of appearance in the fracture showed itself between the interior and exterior folds of the metal.

Effects of the Rivets on the Total Strength of Boilers.

In whatever manner the parts of a steam boiler are united, the rivets which form the junctures being substituted for portions of the metal cut away to receive them, are of necessity so much deduction from the strength of the entire sheet, and unless we can suppose that the strain brought upon the rivet by the manner of setting it, when hot, and in that state making so close a bearing as, when cold, to contract and create a pressure that shall furnish an adhesion by friction between the overlapping surfaces, equal in amount to the strength taken away by the line of rivet holes, we cannot in computing the absolute strength of any steam boiler, safely rely on more than the portion of metal which remains in the intervals between the holes. That the requisite amount of friction could not, under the circumstances which practice assigns to the case, be produced in the manner supposed, is easily demonstrable, since, for that purpose, the friction on two square inches of surface, (the amount which on each side of the line of rivets could possibly be available for this purpose,) must be equal to the strength of the part cut away, which seldom falls short of $\frac{5}{8}$ of an inch. If we could suppose the rivet of this size so strained by hot working and shrinking as to exert $\frac{2}{3}$ of its entire strength or what would be just sufficient to produce in it a permanent elongation, then as its area of section would be .3068 of a square inch, admitting the iron of which it was made to be capable of bearing 55000 lbs. to the square inch, we should have an actual pressure of $55000 \times \frac{2}{3} \times .3068 = 11249$ pounds to be distributed over 4 square inches, giving for each of the two sheets 5624 pounds of pressure, the *friction* of which is to counteract the cutting away of $\frac{5}{16}$ of its substance.

If we suppose the plate to have the same strength per square inch as the rivet, and $\frac{1}{4}$ of an inch in thickness, two-thirds of the strength of the part cut away ($\frac{5}{8}$ of an inch wide,) would be 5726 pounds. From this it is evident that for a weakening of 5726 pounds, we have only the compensation of the *friction* produced by a pressure of 5624 pounds. That fractures not only do take place along the lines of rivets when violent explosions occur, but that incipient fractures and permanent elongations are presented at the spaces between the rivet holes, is evinced by the appearance of the holes themselves elongated by strains and especially by overheating in the vicinity of the juncture. Examples of this are found in Table XCVIII.

In a piece of boiler iron which had been some years in use, and which from overheating, in consequence of the collecting of a quantity of sediment in the part immediately above the fire, had become locally swelled into a protuberance reaching nearly the breadth of a sheet and from nine to ten inches in the direction of the curve, the rivet holes on both sides of the sheet, opposite to the protuberance, were found to be elongated as in the following table. All those on the side nearest to the summit of the protuberance, were found to have cracks running out in different directions, and in some cases, nearly traversing the entire space between the two adjacent rivets. (See the figure, page 238.)

Within half an inch of the first five rivets, and parallel with their line, a strip a, one inch wide, was cut from this piece of boiler, and tried at several filed sections. The report of these trials will be found in Table C. from which it will be seen that the portion nearest to the protuberance, and which had been reduced in thickness by the stretching, possessed much less strength *per square inch* than that which was still of the original thickness of the sheet. The surface of this bar, when the oxide had been removed, presented a distinctly spongy or *honey-comb* appearance, having many minute cells, which being developed by the shears were seen to be nearly filled up with oxide. The committee took an opportunity of making direct trial of the diminution of strength by rivetting. A bar of iron $\frac{9}{10}$ of an inch wide and .134 inch in thickness was rivetted together at a lap six inches long, by 12 rivets, each .205 inch in diameter, in two rows along the length of the bar, but alternating with each other in a zigzag course, the rivet near one edge being about one-third of an inch in advance, along the length, of the preceding one on the opposite edge.

TABLE XCVIII.

Effect of unequal strains on rivet holes.

No. of rivet holes.	Position of the rivet holes.	Length of the major axis of the rivet hole lying in the direction towards the swelled portion.	Length of the minor axis transverse to the swell, and in the line of the series of rivets.	REMARKS.
		Inch.	Inch.	Refer to fig., page 238.
1	At the part of the plate nearest to the swelling.	.750	.700	The centre of the rivets in this piece of boiler iron were 2 inches apart. Two cracks proceeded from this hole, one towards the interior of the sheet 4-10 inch long, the other towards the next rivet, 9–10 inch long.
2	Do.	.735	.650	An oblique crack proceeding gradually towards the interior but more rapidly to the next rivet, 9-10 inch long.
3	Do.	.740	.700	Crack proceeding towards the next rivet ¾ inch in length.
4	Do.	.705	.670	Two cracks proceeding in opposite directions, one 2-10 inch, the other 3-10 of an inch.
5	Do.	.750	.705	These cracks proceeding from the periphery of this hole, each from 3 to 4 tenths of an inch in length.
6	At the part of the plate most remote from swelling.	.725	.700	No cracks observable.
7	Do.	.715	.700	Do.
8	Do.	.675	.645	Do.
9	Do.	.690	.665	Do.
10	Do.	.665	.665	This hole appears to be the only one which retains its original size and shape.

The above described compound bar broke with an oblique section 1.05 inch long, passing through two rivet-holes.

The *strength* computed on the area of *cross* section was 52580 pounds, which reduced to the area of the actual oblique section is 45068 per sq. inch.

The same bar afterwards broken at a point six inches from the foregoing fracture, exhibited a strength of 66027 pounds. If we conceive the diameters of two rivets deducted from the cross section there remains but .49 of an inch in the breadth of the strap of iron which supported an effective strain of 6341 pounds. But the diagonal section of fracture had an area, after deducting the diameters of two rivet holes, of .64×.134=.08576, whereas at the place of fracture, six inches distant, the area was .113529. The effective strain which broke the bar in this latter case was 7496 lbs. Applying this as a standard to the actual section of fracture at the rivets, we have the following calculations :—

1. For the weakening effect of cutting away the metal at the rivets; .113529 : .1407 : : 7496 : 9290=the effective strain which would have been borne by a section of this metal, 1.05 inch wide and .134 inch thick, had none been cut away to make room for the rivets. But the actual area left was

only .08576. Hence .113529 : 08576 : : 7496 : 5662 which ought to have been borne if no strengthening influence had been exerted by the rivets. The difference, or 9290—5662=3628, is the reduction of strength by cutting out the two cylindrical holes; but the actual effective strain was 6341. So that the weakening effect is in fact 9290—6341=2949.

2. The strengthening effect of the friction is 6341—5662=679.

Construction of Cylindrical Boilers and Flues.

In connection with the subject of rivetting it may be remarked, that unless a due regard to the form of the parts of a boiler be observed in constructing it, and a due attention to the rivet holes in the different pieces, the strains incidental to the boiler in its regular use, may be made more dangerous than that which would arise from the mere elastic energy of the steam alone. The usual practice in constructing cylindrical boilers and flues is to make the portions in the form of frusta of cones, the smaller end of one of which entering the larger end of the next, is rivetted in such a manner as to turn all the outer laps in the direction of the *fire end* of the boiler. It is evident that in such a construction the several plates which constitute a single ring of the boiler must have the lines of rivets along their *ends* placed in converging *right lines* and those along the sides or longest edges, in circular arcs of different radii. If in any case these lines are inconsistent with the positions of others to which they are required to conform, the holes must either be rimmed out, or the sheet unduly strained in attempting to force a conformity of positions.

In every case in which a just construction by the aid of previous calculation is obtained, the centres of the rivet holes are the points from which our calculations must commence.

It is obvious that the diameter of the cylindrical, or rather of the compound conical, shell, must constitute one element of the calculation, the breadth of the sheets a second, and the thickness of the metal a third. From these data may be found the radii of the curves in which are to be placed the rivets, and also the convergency of the two straight lines along the two ends of each sheet. The difference in the exterior diameters of the two ends of each frustum must at their respective rivet-lines be equal to twice the thickness of the metal. The larger arc for the rivet holes must manifestly be described by a longer radius than the smaller, and the difference of the two radii is the breadth of the sheet between the curves.

The larger radius will in every case be found *by adding to the mean exterior diameter of the boiler in inches, the thickness of the plate in the same denomination, multiplying the sum by the breadth of the plate, and dividing the product by twice the thickness of the plate.**

Effects of use and long exposure on the strength of boiler-iron.

This topic may be regarded as one of the most important which came under the notice of the committee. To treat it in all its bearings would demand far more of time and means that were at our disposal.

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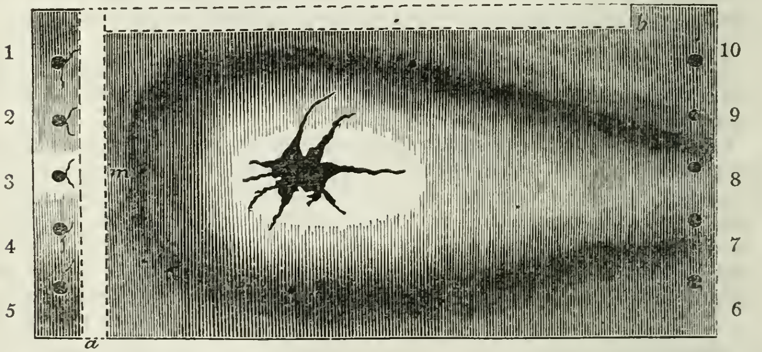
* Thus putting D=the exterior diameter of the boiler,
t=the thickness of the metal,
b=the breadth of the sheet between the two arcs of rivets,
A=the length of the greater arc which will reach round the larger end of the frustum,
a= " less Do.
R=the radius of the greater arc,
r= " less; so that $R—r=b$,
also $\pi=3.1116$.

Table C. contains the results of trials on four bars or strips cut from pieces of plate which had been long in use, and which were more or less visibly affected by the strain, or by the action of water and steam to which they had been subjected. They were cut lengthwise and crosswise respectively from the sheets out of which they were taken, one bar in each direction out of each sheet.

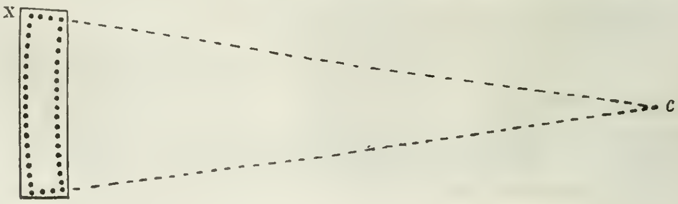
The two strips, Nos. 245 and 246, taken from a specimen of plate which had been burst, are represented in position by the dotted lines in the accompanying sketch. The length of the specimen is the original breadth of the sheet and lay as usual in the direction of the length of the boiler, 30 inches in diameter, from the bottom of which near the fire-end this piece was taken. Hence the shorter strip (*a*) is a length sheet strip, and the longer (*b*,) a cross sheet strip.

Agreeably to what has already been shown, the former possessed the greater strength, but the second experiment upon it made at the point *m*, where the swelling commenced, was found, as shown in the table, considerably weaker than the sections near the two ends of the strip.

The amount of the swelling on this specimen of iron may be seen by the two sketches, page 239, one of which is longitudinal and the other transversal, and the lengths of the ordinates, from lines at right angles to each other, and both passing over the centre of the swelled part, at *b* will be found in the



Then, since the arcs are similar, $A : a :: R : r$ and $A - a : A :: R - r : R$. But $A = \pi\ (D+t)$ and $a = \pi\ (D-t.)$ Hence $A - a = \pi\ (D+t) - \pi\ (D-t) = 2\,\pi\,t$. Hence by the above proportion and putting b for $R - r$ we obtain $2\,\pi\,t : \pi\,(D+t) :: b : R$, from which results $R = \frac{b(D+t)}{2t}$, and $r = \frac{b(D+t)}{2t} - b$. As there must be the same number of rivet holes in each arc, a knowledge of the lengths of the respective arcs and of the number of rivets to be placed round the boiler, determines their distances from each other, on the two curves respectively. The accompanying figure represents the arrangement of the rivet-holes in a plate of metal adapted to constitute a part of a cylindrical boiler, with the centre *c*, and radii *cy* and *cx* of the two arcs of rivet-holes.



accompanying table. The ordinates on the inside or concave part of the plate, commence Fig. 1 from the centre of the rivet holes nearest to the place of rupture *b*. The points on the two axes were 1.075 inches apart.

TABLE XCIX.

No. of mark on the line lengthwise of specimen.	Ordinate or distance of the line from the bottom of the cavity in inches.	REMARKS.	No. of mark on the line transverse to the curvature of the piece.	Ordinate or distance from the co-ordinate to the inner surface of the swell.	REMARKS.
1	.150		1	.700	
2	.500		2	1.425	
3	.945		3	2.200	
4	1.430		4	2.720	The rupture was opposite to these marks.
5	2.000		5	2.960	
6	2.075	The rupture was opposite to these marks and the distances could not be measured with the same precision as the rest.	6	2.320	
7	2.350		7	1.745	
8	1.975		8	0.940	
9	1.870		9	0.390	
10	1.600		10	0.000	
11	1.360				
12	1.115				
13	0.940				
14	0.665				
15	0.520				
16	0.395				
17	0.350				
18	0.265				
19	0.145				
20	.080				

The original distance in the line *ac*, Fig. 1., was $22\frac{1}{2}$ inches. The length of the curve *abc* by actual measurement was found to be 24.6 inches. Hence the extension of the metal in this direction had been 2 1-10 inches in 22 5-10 or $\frac{1}{10.7}$ of the whole length.

Fig. 1.

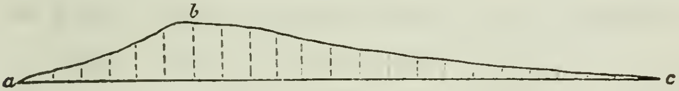
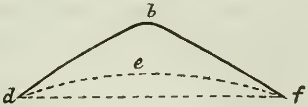


Fig. 2.



The length in the direction of the curve *def*, Fig. 2., was originally 10.6 inches. Actual measurement over the curve *dbf*, gave the length 12 inches, showing an elongation of fibres in this direction equal to 1.4 inches in 10.6 or $\frac{1}{7.5}$ of their whole original length. This greater extension of the fibres in the longitudinal direction of the sheet, accords with what has already been proved by direct trials on bars cut from new plates.

TABLE C.

Experiments on old boiler iron, Nos. 97, 98, 245 *and* 246. *The two former manufactured by Lukens, and the two latter uncertain, all having been some time in use, and visibly affected by the action of heat*

No. of the bar.	No. of the exp't.	DATE.	Breadth of the section of fracture before trial.	Thickness of the section of fracture before trial.	Area of the section before trial.	Temperature.	Breaking weight.	Breaking weight × leverage.	Friction.
97	1	1836. Feb. 13.	.785	.197	.154645	45°	273	8190	409
"	2	"	1.000	.215	.215000	45	349	10470	523
"	3	"	1.100	.100 .200	.165000	45	282	8460	423
"	4	Feb. 27.	.736	.217	.159712	41	285	8550	427
98	5	Feb. 13.	.720	.194	.139680	45	238	7140	357
"	6	Feb. 27.	.705	.214	.150870	41	275	8250	412
"	7	"	1.000	.182	.182000	41	338	10140	507
"	8	"	1.051	.220	.231220	41	373	11190	559
245	9	Mar. 21.	.661	.165	.109065	50	207	6210	310.5
"	10	"	.732	.141	.103212	50	170	5100	255
"	11	"	.731	.181	.132311	50	228	6840	342
246	12	Mar. 26.	.704	.182	.128128	51	206	6180	309
"	13	"	.721	.182	.131222	51	219	6570	328
"	14	"	.686	.182	.124852	51	212	6360	318
"	15	"	.693	.183	.126126	51	231	6930	346.5

TABLE C.

or corrosion. The two latter were from a specimen in which was a rupture in consequence of the collection of a sediment over the part exposed to the fire.

Effective strain.	Strength in lbs. per square inch.	REMARKS.
7781	50314	This strip was cut crosswise of the sheet,—filed section, edges and faces both filed.
9947	46265	Original section produced by the shears.
8037	48103	Fracture at a part deeply corroded. The thinnest place before trial was found but one-tenth of an inch thick.
8123	50860	The edges of this section of fracture were deeply filed, and the oxide was removed from the faces of the bars.
6783	48561	Filed on both edges and faces. Cut lengthwise.
7838	51952	Filed deeply on the edges. Oxide barely removed from the faces.
9633	52928	The edges barely straightened, much filed on the faces.
10631	45982	Edges barely straightened by filing—faces unfiled.
5989.5	54000	Deeply filed on the edges. Oxide removed from the faces. This strip cut lengthwise of the rolling.
4845	46942	This part of the bar had been over-heated, and stretched. Deeply filed as above.
6498	49111	Deeply filed, and oxide removed.
5871	45821	This strip cut crosswise from the ruptured specimen. (See figure on page 238.) Filed and scale removed.
6242	47568	Filed and cleared of oxide as above.
6042	48465	Do.
6583.5	52205	Do.

The two specimens, Nos. 97 and 98, Table C., were deeply corroded in certain parts, indicating the existence of local chemical actions arising probably from inequalities in either the purity, or the mechanical structure, of the sheet.

In both sets of the above trials on old boiler-plate it will be observed that though the mean strength is low, being under 50000 pounds per square inch, yet the principle is still preserved which assigns a greater tenacity in the longitudinal than in the transverse direction of the rolling. The difference, in this respect, between Nos. 245 and 246 is 50017—48515 or 3 per cent. of the strength of the transverse strip.

Between Nos. 97 and 98 the difference is somewhat less.

Effect of Annealing on the tenacity of Iron.

In a variety of cases the committee have endeavoured to extend the range of their experiments so as to embrace the condition of a steam-boiler which without being exploded has suffered from the exposure of its fire-surface when destitute of a due supply of water, to the action of heat above redness. In a few of the trials at elevated temperatures, this point was attained or surpassed, and the subsequent trials on parts near the places of fracture in such cases, gave evidence that the condition of the metal in regard to tenacity had been altered. In the case of bar No. 13, Table XXXI., it will be observed that annealing, which in that instance was produced by the folding over of the ends of the bar to obtain a more certain hold by the wedges, determined at once the place of fracture.

On specimens 199 B and 199 C, Table CII., which were wires manufactured at Phillipsburg from Juniata iron, were made several experiments to ascertain its mean tenacity, in the ordinary state of the article. On other pieces of the same wires the process of annealing at high temperatures was performed. The results show that the maximum effect of annealing on one of the sizes was a diminution of 27.5 per cent., and on the other 46 per cent. of the original strength.

In these instances the annealing was performed in a common smith's fire, without using any other precaution to defend the wire from oxidation than merely covering it with cinder.

This prevented, for the most part, any action of the air on the wire during the short time of its remaining in the fire. But to obviate altogether the objection that oxidation might have some share in producing the weakening effect, several specimens of iron were, after being carefully gauged, weighed and, having their specific gravities taken, rolled up in several folds of clean sheet iron; the ends of the folds turned over and hammered flat, to prevent access of air, and the whole then exposed for 15 or 20 minutes to the blast of a smith's fire, gradually raising it to a very high *welding heat.* In this state the package was withdrawn from the fire, often exhibiting one or two folds of the sheet iron cut through by the blast, but in no case extending to the enclosed specimens. When taken from the fire it was immediately buried in dead cinders, where it was allowed to remain until quite cold. The wrapping of sheet iron was then removed, and the specimens generally found with no visible change except a slight discolouration by black oxide of the metal, insufficient, however, to affect sensibly the weight of the specimen.

The five trials on Nos. 224 C and 254 D were among those which had been reduced or *constricted* by straining before the specimens were annealed.

TABLE CI.

Wrought iron annealed at different temperatures.

No. of the comparisons.	No. of the specimen on which the trial was made.	No. of the table containing the results on each specimen.	Strength at ordinary temperature before annealing.	Temperature at which the annealing took place.	Strength at the annealing temperature.	Strength after annealing and cooling.	Rate of diminution in strength by annealing.
1	152	LI.	57133	1037°	37764	55678	.025
2	214	LXII.	59219	1022	37410	46612	.213
3	227	LIV.	53774	1111	27604	52186	.029
4	226	LIII.	52040	1142	18672	44720	.140
5	227	LIV.	53774	1155	21967	45597	.152
6	229	LVI.	53185	1159	25620	46212	.131
7	227	LIV.	53774	1187	21919	45027	.162
8	226	LIII.	52040	1192	29703	43154	.170
9	226	LIII.	52040	1237	21298	44165	.151
10	226	LIII.	52040	1245	20703	38843	.253
11	224C.	LXXII.	48407	Bright welding heat.		36052	.255
12	224C.	LXXII.	48407	Do.		39333	.187
13	224D.	LXXIII.	48830	Do.		35889	.265
14	224D.	LXXIII.	48830	Do.		36706	.248
15	224C.	LXXII.	48407	Do.		38676	.201
16	19	XXXII.	52912	Do.		44191	.165
17	199B.	CII.	73880	Low welding heat.		53578	.275
18	199A.	LXIV.	76986	Bright welding heat.		50074	.349
19	199C.	CII.	89162	Low welding heat.		48144	.460

TABLE CII.

Experiments on two specimens of wire, Nos. 199 *B. and* 199 *C.* }
Manufactured at Phillipsburg, Pa., from Juniata iron. One specimen }

No. of the exp't.	Diameter of the wire.	Area of section of the wire before trial.	Temperature when tried.	Breaking weight in the scale.	Breaking weight × leverage.	Friction of the machine.	Effective strain.
	inch.	square inch.					
1	.190	.0283526	70°	73.	2190	109.5	2080.5
2	do.	do.	70	73.	2190	109.5	2080.5
3	do.	do.	70	74.5	2235	111.7	2123.3
4	do.	do.	70	74.	2220	111.	2109.
5	do.	do.	70	73.	2190	109.5	2080.5
6	do.	do.	76	49.5	1485	74.2	1410.8
7	do.	do.	76	54.5	1635	81.7	1553.3
8	do.	do.	76	62.	1860	93.	1767.
9	do.	do.	76	61.5	1845	92.2	1752.8
10	do.	do.	76	61.5	1845	92.2	1752.8
11	do.	do.	76	52.	1560	78.	1482.
12	do.	do.	76	53.5	1605	80.2	1524.8
13	do.	do.	76	52.5	1575	78.7	1496.3
14	do.	do.	76	54.	1620	81.	1539.
15	do.	do.	76	54.5	1635	81.7	1553.3
1	.156	.0191127	76	60.	1800	90.	1710.
2	do.	do.	76	60.	1800	90.	1710.
3	do.	do.	76	59.5	1785	89.7	1695.3
4	do.	do.	76	60.	1800	90.	1710.
5	do.	do.	76	59.5	1785	89.7	1695.3
6	do.	do.	77	32.	960	48.	912.
7	do.	do.	77	32.5	975	48.7	926.3
8	do.	do.	77	32.5	975	48.7	926.3
9	do.	do.	77	33.5	1005	50.2	954.8
10	do.	do.	77	33.5	1005	50.2	954.8
11	do.	do.	77	34.5	1035	51.7	983.3
12	do.	do.	77	35.	1050	52.5	997.5

TABLE CII.

{ of each size, broken up cold, and without annealing, the others annealed and cooled, either in cinders or in water.

Strength in lbs. per square inch.	Mean strength in each state.	REMARKS.
73379		The first five experiments were made on specimen 199 B, of the wire, unannealed.
73379		
74880		
74386		
73379		
	73880	
49760		This and the four following experiments were made on a specimen of the wire of about the same length as the above, annealed by heating to redness, and then buried in dry ashes until cold.
54786		
62323		
61819		
61819		Broke in the gripe of the wedges.
	58101	
52268		This and the four following were made on a specimen of the same wire as the two above mentioned, annealed at redness and immediately quenched in cold water.
53781		
52775		
54281		
54786		
	53578	
89469		This and the four following experiments, were on the smallest size of wire, 199 C., in its unannealed state.
89469		
88703		
89469		
88703		
	89162	
47714		This and the two following were made on wire of the same size, and from the same piece as the above, annealed and buried in ashes.
48360		
48360		
	48144	
49958		This and the three following were on the same size of wire, annealed and immediately quenched.
49958		
51449		
52192		
	50889	

No essential change of specific gravity from annealing was detected, except in the case of a piece which had been beforehand excessively hammer-hardened. This appeared to have been slightly diminished in density by the annealing process. When the specimens treated in this manner had been previously strained nearly to the limit of their tenacity, and their original areas of section consequently much reduced, the precaution was taken to regauge them before annealing, so that we could employ the then existing areas of section, instead of the original ones, as the basis of calculation for the strength per square inch. This course was followed with Nos. 224 C and 224 D.

Calculating the strengths on the areas of these bars taken just before annealing, they were found as follows:

Instead	of	36052	we	obtained	45090,
"	"	39333	"	"	41980,
"	"	35889	"	"	42700,
"	"	36706	"	"	43200,
"	"	38676	"	"	41980.

From table CI. it appears, that, computing the strength in the comparisons from 11 to 15 inclusive on the area before annealing, and excluding Nos.1 and 3 of the series, as not made at a sufficiently high temperature to effect the purpose, we get the mean tenacity of iron, by seventeen comparisons after annealing, equal to 45117 lbs. to the square inch.

In a considerable number of cases where trials were made at high temperatures, especially those above 1000°, the iron was left in an annealed state. In several of these the effect of the process is nearly as striking as where the heating had extended to the point of welding.

In other cases the difference between the strength previous to annealing, and that exhibited afterwards, was so small, that it was difficult to refer it to any other cause than the original inequalities of structure.

It will be seen that, from Experiment 3 to Experiment 11 (Table CI.) the loss of tenacity by annealing follows very nearly the order of the temperatures at which the process was performed.

That boilers do, in the course of ordinary practice, frequently become annealed there can be little doubt. The four specimens of old boiler-iron already spoken of, indicate clearly the existence of such a state. The mean strength of those specimens by 15 experiments was 49278 lbs., while the mean minimum strength of the four bars was 46252, quite within the range of the tenacity of iron annealed at a red heat. Hence, unless we can be certain that a boiler will be entirely secure from this process, we shall not be warranted in calculating its strength at any greater amount than about 46000 pounds per square inch, and of this amount but two-thirds can be assumed as a safe basis of calculation, being that at which permanent change of form would take place.

Your sub-committee have at length brought to a conclusion the research which has so long* occupied their attention. The foregoing pages present as concisely as the case seemed to admit, the facts which their investigations have elicited, in regard to the several inquiries, Principal, Incidental and Subsidiary, stated in the preliminary part of this report. The great interest of the subject required that the utmost care should be exercised to prevent error. Hasty and unwarrantable conclusions were to be avoided. It has been our sincere desire to meet every just expectation in regard to the practical usefulness of the information of which we were in search. It would be presuming too far to suppose that in so extensive a mass of calculations we had escaped all error. So many steps of each process have, however, been preserved, that verifications will be easily made. Speculations, merely scientific, have been deemed inappropriate to this report, and we now abstain from swelling it with repetitions of statements already made, or with any matters not strictly pertinent to the subjects embraced within the views of the general committee.

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* This sub-committee was appointed January 4, 1831, and preparations for the experiments immediately prosecuted.—The trials of tenacity were commenced April 4, 1832. The last experiments were made January 5, 1837.

WALTER R. JOHNSON.
BENJAMIN REEVES.

N. B.—Prof. A. D. Bache, the remaining member of the sub-committee, being absent from the country at the time of making this report, we regret that it is impracticable to submit it to him for inspection and signature.

In the course of this inquiry we have been indebted for valuable personal assistance to several friends of the useful arts, among whom we would particularly mention the late Benjamin Say, Esq., and Messrs. James M'Crea, James Barnet and Joseph Brezinski. The last named gentleman repeated and verified many of the calculations as well as aided in various experiments.

Note.—Mr. Telford has made an interesting series of experiments on the strength of wire, an account of which is found in Mr. Barlow's treatise on the strength and stress of timber, page 254.

The same gentleman made various experiments on bars and bolts of iron, detailed in the same work.

He also attended to the successive elongations of bars under different weights, and noted the amount of recoil or contraction when relieved from strain.

Captain S. Brown has also furnished to Mr. Barlow, a series of highly interesting experiments on the strength and elongation of iron bolts.

The mean result of three experiments by Mr. Brown on cast-iron was 18564 lbs. to the square inch.

Mr. Hodgkinson has published in the third report of the British Association, three results of experiments on the same material, which make its strength 17136 lbs. per square inch, while the three experiments of the committee of the Frank. Inst., which were considered fair, indicated in the bars a strength of 20834 lbs.

Mr. Brunton and Mr. Brunel, have each engaged in this interesting department of inquiry and given the results of experiments on a large scale which will be found in the work of Mr. Barlow already cited.

Mr. E. Martin, formerly of the Polytechnic school, has given in the Annales des Mines, Vol. V., a series of experiments executed in France, under the orders of M. Barbé on round rods of iron, 18 or 19 feet long and 2 inches or more in diameter, made with a view, in part, to determine the recoil when released from strain, and the actual amount of elongation under each weight to which it was subjected.

In the same volume of the Annales des Mines, M. Vicat has a paper referring to, and controverting some of the positions of M. Martin, but not affecting the statements respecting the experimental operations just referred to.

In the same work, Vol. VI., are contained some interesting statements by M. Payen, respecting the manufacture of wire, in which the relative ductility before and after annealing is established.

In the Annales de Chimie et de Physique for Sept. 1833, is a valuable paper by M. Vicat, showing the influence of time on the gradual extension of wires under different weights. Each of his experiments occupied nearly three years.

The quantity by which iron extends under different degrees of tension, and on the recoil when relieved from strain, has been examined by Prof. Barlow. See Journal Fran. Inst. Vol. XVI. p. 124, &c. The relation between the effect of straining and elongating a bar by mechanical means, and that of expanding it by heat, is also noticed.

Physical Science.

Experiments on Solar Light. By John W. Draper, M. D. Professor of Chemistry, Hampden Sidney College, Va.

(Continued from p. 38.)

68, *List of metallic salts changed by light.* 69, *Analysis of the dark chloride of silver.* 73, *Action of moonlight and artificial lights.*

74, *Perihelion motion of matter.* 76, *Doubtful if it be always the same.* 77, *Modes of making these experiments.* 80, *Dew of water.* 81, *Dew of mercury.* 82, *Iodine.* 83, *Chloride of gold.* 84, *Non-deposition on a glass plate.* 86, *Current action.* 88, *Action of terrestrial flames.* 91, *Temperature of sides of a jar.* 92, *Modification of light by reflexion.* 93, *Action of a metal screen.* 95, *Protecting action of a metallic ring.* 102, *Action of non-conducting bodies.* 104, 106, *Electricity obtained from the solar ray.* 110, *Explanations founded on that hypothesis.* 112, *Determination which class of rays causes this perihelion motion.*

68. The chemical rays, thus closely attending the luminous rays, and being like them subject to the forces bringing about reflexion, refraction, and inflexion, it would become a matter worthy of inquiry, to find whether there be any different classes of these rays, analogous to the different coloured rays of light, or the unequally refrangible and absorbable rays of heat. The salts of silver, are only one of a class over which the chemical rays exert an action. The following list contains, I believe, all the metallic salts, at present known, in the constitution of which changes are brought about by exposure to the sun.

Chloride of manganese,
Sulphocyrate of iron,
Sulphate of nickel,
Carbonate of lead,
Carbonate of nickel,
Nitrate of bismuth,
Chloride of uranium,
Sulphate of uranium,
Nitrate of uranium,
Chloride of copper,
Iodide of mercury,
Chloride of mercury,
Bichloride of mercury,
Chloride of silver,
Bromide of silver,
Sulphocyrate of silver,
Nitrate of silver,
Bromate of silver,
Chloride of gold,
Chloride of osmium and potassium.

Besides which, there are two others whose constitution is not well known; one prepared from an alcoholic solution of the double chloride of platinum and sodium, by the action of chloride of potassium, and the other in a similar manner from the cyanide of platinium.

69. The changes which these bodies experience, are of different kinds, some become black and some bleach; some, as the sulphate of nickel, undergo change of crystalline arrangement. If we are to take the chloride of silver as a type of those bodies in this list, which undergo partial reduction, it will be found probable, that the change impressed on them is only superficial, as analysis will show. But we cannot tell with certainty, whether a perfect reduction of some of these compounds takes place, or whether it is a subsalt of a dark grey colour that results. By taking advantage of the property which chloride of silver possesses, of subsiding very slowly from neutral solutions, so as to make them assume a milky consistency, we may present it in a state extremely favourable to the action of the solar ray. For if a thick mass alone be exposed, the central parts will not undergo the same change as the exterior, being shielded by them

from the sun. A milky solution like this will, after an exposure for a certain time, become quite clear, the chloride precipitating, owing to the liquid becoming acidulous. Mechanical agitation being then resorted to, to expose fresh surfaces of the precipitate to the sun, very frequently during a period of eight or ten days, and care being taken to suffer no dust or other impurity to enter the vessel, it will be found that the powder has become of a reddish grey, interspersed with little particles of unchanged white chloride; these, from their superior density, will have precipitated more readily than the grey particles; washing and decantation will therefore readily effect a perfect separation of them. One hundred grains of the dark chloride thus treated, will yield an analysis 79.3 of metallic silver; that quantity contains therefore 20.7 of chlorine, it has lost then by exposure 5.3 grains of chlorine, of the quantity originally contained in it.

70. Other analyses of the same sample, furnished results not widely varying from this, but such is not the case with analyses of different samples, these give sometimes more, sometimes less, chlorine, they prove that the chloride of silver as darkened by light, is not a definite compound, but rather a mechanical mixture; that the change of composition is chiefly confined to the surface, and does not affect the interior of the particles to any extent; it is true, that microscopic observation shows them to have an uniform consistency and colour, but of course reveals nothing of their internal character. An error is frequently made by writers who describe the changes happening in this partial reduction; it is not, as they say, hydrochloric acid which is evolved when the chloride is under water, but it is chlorine, as is made very evident by the strong disagreeable odour of that gas when the experiment is conducted in close vessels.

71. In addition to the list given above of substances changed by the chemical rays, there are some others which exhibit their energy in a very marked manner. Chlorine and hydrogen unite together with an explosion; carbon and chlorine are also made thus to unite, in producing the per-chloride of carbon; all kinds of vegetable colours are bleached; hydrodide of carbon and chloro-carbonic acid are always made by the action of solar radiant matter.

72. It has been stated by some chemists, that whilst the violet extremity of the solar spectrum blackened cloride of silver, there are other parts of it which would bleach the salt so blackened; but it is not so, for neither does any part of a very dispersed spectrum, nor the rays which have passed through a variety of absorbing media, exert such an action. These experiments I tried repeatedly, under all the conditions of variation of temperature and brilliancy of the solar rays, but no observation led to the inference that there was any change of colour, or any sign of an approaching change, even after the lapse of a whole month. Indeed, it would seem that the state of this case does not justify any such expectation; when the chemical rays have disunited the chlorine, it is gone and lost forever to the silver, being scattered abroad in the atmosphere; if therefore, the substance ever regains a white colour, chlorine must have been purposely furnished from other sources, or the white substance said to result, is some compound of unknown ingredients.

73. The light of the moon is a remarkable example of luminous rays existing without either calorific or chemical rays; the most delicate thermometric arrangements have hitherto failed to show any rise of temperature in the moonshine. A piece of paper, imbued with chloride of silver, may also be exposed to the rays of the full moon, converging from a glass, and it will not exhibit any change; this I proved, by placing such a paper in a

situation where for a whole night the rays of the moon could reach it. And the same observation applies to terrestrial flames. In none of these has the existence of the chemical rays been detected. Chloride of silver, after being exposed for eight hours to the bright flame of an argand lamp, converged by a lens, retained its whiteness. The same effect was witnessed, when the flame of alcohol tinged red by strontian was employed, or the yellow flame produced by chloride of sodium, and the green of Boracic acid; in these cases the periods of exposure did not exceed half an hour.

74. OF THE PERIHELION MOTION OF MATTER. Probably the most remarkable effect exhibited by the solar rays, is the motion they produce in media endued with much mobility. For many years it has been known, that camphor exposed in a bottle to the rays of the sun, formed a crystallization on that side of the vessel nearest the luminary; but the action is so slow, and requires such a length of time for its completion, that no successful investigation has been made as to the nature of the forces in operation. Some philosophers have assumed, upon insufficient grounds however, that the crystallization was effected on the most illuminated side, merely because it was the coldest, as we know that vapours are always deposited on that part of a surface whose temperature is the lowest.

75. About three years ago, I published a series of observations on this point, in the Journal of the FranklinInstitute, Vol. XV., p. 156. Having found from some theoretical considerations, that the crystallization of camphor took place in vacuo, with a rapidity convenient for experimental investigation, I was led to make an extended inquiry into the whole matter. The results so obtained, are now given to the public, they appear to me to be so singular and important, and to conceal some secret respecting the physical constitution of the sun's rays, that I cannot doubt they will lead to a rich harvest of discovery.

76. The sun's rays have the power of causing vapours to pass to the perihelion side of vessels, in which they are confined, but, as it would appear, not at all seasons of the year. For example, I have a certain glass fitted up for making these observations, and in this vessel, during the months of December, January, and part of February, 1836—37, a deposit was uniformly made towards the sun; during the months of March, April and part of May next following, although every part of the arrangement remained to all appearance, the same, yet the camphor was deposited on the side furthest from the sun. From May until the present date, the deposit is again towards the sun. It does not appear that any immediate cause can be assigned for this waywardness. Does it exist in the sun's light? or in changes affecting the earth's atmosphere? or in imperceptible changes in the instrument with which the observation is made? as respects the latter, I think a negative answer may be given without any hesitation; but beyond a mere expression of the fact that these anomalous circumstances do occasionally occur, I would not be understood to speak decisively; if periodic changes like this do occur, which is doubtful, they have not been watched for a sufficient length of time, nor have I made sufficient variations in my trials to be able to refer them to any distinct cause. A large bottle containing camphor, which has been deposited therein for more than a year under ordinary atmospheric pressures, has uniformly showed a crystallization towards the light.

77. For making these experiments properly, it is necessary to possess an air pump receiver ground so true as to be able to maintain a vacuum for several hours, or even days. A less perfect jar may be made to answer,

by fastening it down to the pump plate with cap cement, it will however be liable to leak when the cement becomes warm by exposure to the sun. For many of these trials, a barometer tube is sufficient. Those who are provided with a good pump and jars accompanied with their proper transfer plates, will have no difficulty whatever.

78. Upon the plate of the pump, or one of the transferers *a a* Fig. 1, Plate II., place some camphor in a watch glass *c*, supported by a stand; over this place a bell-jar, and exhaust until the difference of level of the syphon guage amounts to half an inch or less, the further the rarifaction is pushed the better; remove the arrangement into the sunshine In the course of five minutes, if the atmosphere be clear and the sun bright, small crystalline specks will be found on the side nearest to the sun, these continually increase in size, and at the end of two hours, many beautiful stellated crystals, from one-eighth to half an inch in diameter, will be found on that side, but on the other parts of the glass, only a few straggling ones here and there. This appearance, is represented in Fig. 2. Sometimes, as is the case in a result which I keep by me, the whole side next the sun is covered with a lamina of camphor, the other side containing none at all.

79. Or, having made a torricellian vacuum, in a tube upwards of 33 inches long and five-eighths wide, pass into it a piece of camphor, which will rise into the void. This arrangement, like the former, when kept in the dark shows no crystallization, even though so kept for more than four months, but on bringing the vacuum into a beam of the sun, crystallization rapidly goes on, and at the end of a quarter of an hour the appearance is such as represented in Fig. 4. It is not important that the temperature of the sunbeam, or of the atmosphere, should be high; this is an experiment which will succeed at temperatures varying from 120° Fah. to 60° Fah., and probably at much lower degrees, for it is readily performed in the depth of winter.

80. It is in no wise a phenomenon connected with the process of crystallization. Take a jar twelve inches high, and four in diameter, quite clean and dry, place it over a glass of water *b*, fig. 5, and expose it to the sunshine. In this experiment, it is not required that there should be a vacuum within the jar. In the course of an hour or two, there will be a copious dew at *a*, and on further exposure drops of water will trickle down the side of the glass, but on the opposite side not the least cloudiness will be found.

81. Barometers, hung up in such a position that the sun's rays can have access to them, exhibit an analogous appearance on the side nearest the light, being studded with metallic globules.

82. In any of these experiments iodine may be substituted for camphor, provided mercury is not present, nor any other substance on which this material acts; the most advantageous method of using iodine is by heating it in a suitable vessel, and when the vessel is quite full of vapour, presenting it to the sun's ray, deposition goes on, on the perihelion side, as the condensation takes place.

83. Nor is it requisite in obtaining these results, that the material should be either gaseous or vaporous. The rays of light, have the property, as was found by Count Rumford, of decomposing an aqueous solution of chloride of gold; on making this experiment in a test tube one-third of an inch in diameter, as *a* Fig. 6, small spangles of metallic gold will be seen, by reflected light, on the side towards the sun *b*, by transmitted light it appears of a pale green tint, as is the colour of gold leaf. Here we find, that under

certain circumstances, solutions will deposit metallic matter, in obedience to the same laws which cause the crystallization of camphor, and the deposite of aqueous dew.

84. A few pieces of camphor were laid on the plate of an air pump, and a circle of glass two inches in diameter, *a* Fig. 7, was supported on a pedestal in the midst of them, the upper part of the glass being four or five inches above the pump plate; it was then covered with a jar, and exhaustion performed. On exposure to the sun for a suitable length of time, numerous crystals were found on the jar, but none on the circular plate, although it had received the full beams of that luminary. This experiment was made with a view of determining what peculiar condition a glass surface was placed in by exposure to the light; for experimental purposes the rounded form of the glass receivers being very unsuitable, it was not therefore without surprise, I observed that, however long the plate was continued in the beams of light, no crystallization would ensue. A flat surface, however, being essential to the trains of experiment pursued, trials were repeatedly made, by various changes in the arrangement, to cause a deposition of camphor upon such a crown glass plate; but though in five days I could procure starry crystals upon the bell jar of more than half an inch in diameter, in no instance was a solitary one found on the glass plate.

85. Two circumstances may determine the precipitation of camphor crystals on a surface; 1st, Degradation of temperature; 2d, Increase of pressure. To the former we cannot look for an explanation in the case before us, for there is an actual increase of temperature in every part, and more especially in that side of the vessel which is next to the sun. Why then does this condensation take place on the hottest surface, the side nearest to the sun? we cannot admit, that the rays of heat have any active part in bringing about the phenomenon. On the other hand, they ought rather to exert a contrary effect, antagonizing the powers that solicit the camphor crystals to form, and driving them to the coldest surface. We are therefore reduced to the supposition, that when the light of the sun impinges on a surface of glass, it places that surface in such a condition, that it exerts a pressure on the adjacent medium, immediately followed by a condensation of that medium. The state of the force here spoken of, applies to the glass surface alone; it is not an action between the solar ray and the powers that effect crystallization, seeing that it equally takes place in the deposite of aqueous or mercurial dew, and even of solid gold from a solution of its chloride. In other words, if a ray of the sun be incident on a surface of glass, it develops a force of attraction on that surface.

86. A gaseous medium, having its temperature disturbed at any point, has a current determined in it. In a chamber, such as the bell of an air pump, this current circulates round the walls, ascending on the hot and descending on the cool side; it might be supposed, that to this circumstance was due the fact of no crystals being found on the plate of glass, sect. 84. The condensation cannot however be attributed to this cause; for if so, a lamp, or any other source of heat, would be equally effectual, it will however be hereafter shown, that terrestrial flames tend to remove these depositions from the side nearest to them, and cause them to be accumulated in the colder regions.

87. Beneath a receiver, *a* Fig. 3, a cubical bottle *b*, having flat sides, was placed, and in the bottle a few pieces of camphor, the mouth of the bottle was about half an inch in diameter, and was left open, the pressure of the atmosphere being reduced to 1½ inches of mercury. Temperature of the

ray 57° Fah. On examination after the lapse of one hour and twenty-five minutes, no crystals whatever could be found on the receiver, and but a few sparsely scattered on the sides of the cubical phial. Now there can be no doubt that the whole receiver was full of camphor vapour, and it does not appear, that any reason can be assigned for the anomaly of its non-crystallization.

88. Will artificial light produce analogous results? To ascertain this, I took a glass globe about one inch and a half in diameter, with a neck four inches long, fitted it with a stop-cock, and introduced within it a drop of water. The vapour of this water exhibited extreme mobility; the light from the clouds caused its immediate deposition. A further advantage was gained by the use of this apparatus, for by heating the globe uniformly, until all the moisture on its surface was vapourized, and then allowing it to cool, the particles of water readily obey the forces that solicit them. This glass globe, supported vertically on an appropriate stand *a*, Fig. 8, plate II., was placed at a distance of nine or ten inches from a brightly burning argand lamp A; to protect it from accidental currents of air, and from irregularities of radiation from other sources, the whole arrangement was covered by a bell *b c*, open at both ends, and about fifteen inches high. It appeared at first that a thin dew lined the inside of the whole globe, instead of being confined to one part, but after a certain space of time, the heat which passed from the lamp through the protecting glass, disturbed the results, the dew being driven to the coldest parts. To get rid of the effects of this heat, at a distance of about three feet from the lamp A, Fig. 9, a double convex glass lens *c*, 2½ inches in diameter, was placed, which brought the rays to a focus at a distance of five or six feet, where stood the glass globe *a*, covered with its protecting jar. The globe had been previously slightly warmed, so as to expel all the dew from its surface, and give it an uniform temperature; in several trials it was found, that there were no evidences that the bright flame of an argand lamp exerted any force soliciting the vapour of water to move towards one part of the glass, rather than another.

89. I took the arrangement of section 80, and shut it up in a dark closet, having previously made the jar perfectly clean and dry; it remained there for several days, that it might be found whether those little irregularities of temperature which occur in such confined chambers, would cause this dew to pass to one side of the glass rather than another; it did not appear that such was the case, for the glass was as free from moisture when taken out, as when shut up. And now, this arrangement being placed in the window, where the sun was brightly shining, exhibited on its perihelion surface in the course of three and a half minutes, a pearly dew; and in six minutes drops of water were trickling down that side.

90. But it is not essential to the success of this last experiment, that the solar ray itself should impinge on the vessel. The temperature in the shade being 94° Fah., I placed the receiver with its cup of water in a window having a northern exposure, and found that the dew readily made its appearance on that side which was towards the light.

91. It has been suggested by some who have seen these experiments performed, that when a glass vessel is exposed to the sun, that part of the glass which is nearest to him, may actually be the *coldest*; such an opinion it is evident rests on no sufficient grounds; for the sake however of those who see force in this objection, the following experiment was made. A jar *a g*, Fig. 10, was taken, of such dimensions that it could receive the differential

thermometer *c d b*, the balls of which *b* and *c*, touched the opposite sides, and in the dark the liquid stood at zero, but on bringing it into the sunshine, if the side *a* was exposed, then the ball *c* was warmest, and if the side *g* then the ball *b* was warmest, as was indicated by the motion of the liquid. Hence we know, that in all cases where crystals of camphor, dew of water, &c. are deposited on the side next the sun, they are so deposited in spite of an energetic force, which tends to remove them.

92. Light which has suffered reflexion at certain angles, appears to have undergone a remarkable modification, being no longer able to put the glass into such a condition that it can cause motion towards the sun. It is not to be inferred that any connexion is here traced between this disturbance of the condition of light, and the change impressed on it by polarization. A beam of the sun falling on a plate of glass, and being reflected at an angle of 45°, may be intercepted by any of the arrangements of sections 78, 79, as by the barometer tube. It will be found that the crystallization proceeds with considerable rapidity, not however on the perihelion side of the vessel, but on the opposite side. It is probable that this result is not dependent on the polarization of light, inasmuch as it takes place equally well at all the angles, less and greater than the maximum angle of polarization of glass. A ray of the sun cannot be made to disappear entirely, as is well known, by any disposition whatever of two reflecting glass plates, though the pale light shed by the clouds, may be very nearly brought to that condition. But light, even that of the sun, having once undergone reflexion, has received some determinate impress, which disables it entirely from causing camphor to crystallize on the perihelion side of vessels.

93. Another very remarkable phenomenon, is exhibited by the following arrangement. Take a receiver *a*, Fig. 11, twelve or fifteen inches high, and three or four in diameter, place it as usual upon the transfer plate, with its proper charge of camphor *c*. Then cover it with a tin cylinder *e f* of sufficient dimensions, to the end that all the light may be shut out, except at one point *g* where there is a hole, half or three-quarters of an inch in diameter. Under favourable circumstances, as a serene sky and bright sun, let the arrangement be exposed so that a column of light may pass through the aperture *g*, into the glass, it may or it may not finally fall on the camphor at *c*. It would of course be expected, that a collection of crystals would form on the inner surface of the glass, corresponding to the aperture *g*. But on trial it is not so; for however bright the sun may shine, or however favourable other circumstances may be, not a solitary crystal will make its appearance, either there or on any other part of the vessel, provided its temperature has been pretty uniform. On an exceedingly calm and serene day in July, 1835, when every circumstance seemed propitious, I made trial of this matter, and because the jar that I was using was not ground sufficiently true to fit the transfer plate accurately, it had been fixed thereon with common cap cement, and on exposure to the sun, the temperature of the whole arrangement rose so high, that the cement was in almost a semifluid condition; it was one of those days when the eye cannot behold the sky, or look on the ground, without pain, yet not one crystal could be made to appear opposite to the whole. But on taking off the metallic screen, and exposing the jar, in a little more than a minute, small specks were observable on the glass, and in a quarter of an hour, its perihelion side was densely coated with crystals. How are we to explain this? Do the edges of the aperture *g* impress any change on the passing light? Or is the glass surface placed in such a condition, that it can no longer solicit

the deposit of crystals, we shall see hereafter that there are circumstances yet more remarkable, which put us in possession of an explanation.

94. For the proper understanding of the rationale of these experiments, it is required to know, whether it be essential that the solar ray should impinge on the camphor or not; or whether the action is spent on the vapour only. A tube was therefore taken of suitable dimensions, in the lower part of which a fragment of camphor was deposited, and screened as much as possible from the rays of the sun, whilst the upper part of the tube was freely exposed. Crystals formed without difficulty, at a distance of three or four inches, or even a foot, from the camphor, but there appeared to be a limit beyond which they did not readily pass. A tube four feet six inches long, and two inches in diameter, being exhausted, did not show on its exposed end any appearance of crystallization. Near the camphor the deposit was pretty copious, but in advancing from it the crystals were more sparsely scattered, until towards the upper extremity none could be seen. Now the maximum quantity of vapour that can exist in a void, or among other gases, provided the mixture be in æquilibris, depends on the lowness of the temperature of any one part of the vessel, and hence a long tube one of whose extremities is kept cold, does not exhibit these configurations readily, because the quantity of vapour in it is small, owing to the coldness of one part of the void space. It is not necessary, therefore, that the sun should shine *on* the camphor, the effect of the rays taking place entirely on the vapour filling the void.

95. I now come to develop a singular action which certain bodies exert over this process. Take a receiver, able to maintain a vacuum for some time, and having cut out a ring *a*, Fig. 12, Plate II., of tin foil, an inch and a half or thereabouts in internal diameter, and half an inch wide, paste it upon the receiver as at *a*, Fig. 16, moreover, accommodate the receiver with its camphor as usual, and having exhausted, expose it to the direct rays of light, so that the ring *a* shall be on the perihelion side. In the course of a short time that surface will be found studded in various directions with crystals, as is to be expected; but it will be found that none of these crystals trespass within a certain distance of the ring, and that not one is to be seen within the circle circumscribed by it. The ring, therefore, exerts a kind of protecting action on the glass, forbidding the deposition of crystals within certain limits; such a result is depicted in Fig. 13.

96. This action of a ring, formed of good conducting materials, might be supposed to arise either from its adding something to the surface of the glass, or taking something away from the glass with which it is in contact. Or, on the other hand, it might be imputed to some change impressed on the ray of light. Take therefore a ring *a*, Fig. 14, and place it *before* the receiver *b*, at a distance of half an inch, the ring being of the same dimensions as in the last experiment, it will be discovered that although the ring does not *touch* the glass, it still protects it, no crystals coming within a certain distance of the regions overshadowed by the metal. Nay, even at a distance from the line of shadow, not a crystal is to be seen, nor are any visible in the illuminated centre.

97. Even after crystals have been formed on the surface of the jar, if it be placed in the sunshine with a ring before it, as in the foregoing experiments, the ring will be found not only to exert a protection on the glass, hindering any further deposit, but will even *remove the crystals that are there.*

98. This is indeed a remarkable circumstance; a part of the perihelion

surface is shaded from the sun, and thereby rendered cooler, yet the crystals deposit themselves on the hottest surface, and avoid that where it is cold. I know of none of the commonly received doctrines, that will give the shadow of an explanation of the matter. We see, however, how it happens that in the experiment of admitting a column of light through a hole in a screen, no crystalline deposite was effected, the protecting agency of the metal, whatever its power might be due to, seemed to hinder it.

99. To give the particulars of one of these experiments. On the 11th of July, I prepared an arrangement, such as the foregoing, the thermometers in the shade were at 76° Fah., and in the sun at 99° Fah., distance of the ring from the jar half an inch, its natural diameter .75, width half an inch. After proper exposure, the jar was examined, there were no crystals on that part opposite the central opening of the ring, and the nearest crystal to the natural border was $\frac{6}{10}$ inch distant from where the shadow was projected on the glass.

100. Vapour of water exhibits similar phenomena, a thin lamina of tin foil in the form of a cross, a ring, or any other shape, effectually prevents the deposit of water near it.

101. Instead of placing the ring outside of the glass, now let it be placed on the inside, as at *a*, Fig. 15, so that it may be within one-eighth of an inch of the surface. When the crystals have fully formed, it will be discovered, that the ring has exerted the same kind of protecting agency that it did when on the outside of the glass.

102. Hitherto, a class of bodies has been tried, as protectors, which are without exception good conductors of electricity, such as the metals. Certain indications led me to make trial of resinous matters, which are non-conductors of electricity. Having made the region about *a* fig. 16, of the air-pump jar, very warm, over a spirit lamp, a ring of rosin was spread on it, about the same size as the ring of tin foil, which had been formerly there. This ring of rosin was transparent, admitting the light to pass it readily, and at a certain distance appeared of a fair amber colour. Having arranged the jar as usual and exposed it to the sun, after a certain length of time well marked crystals were deposited on the perihelion side, on which the rosin was; these crystals not only came up to the verge of the rosin and filled also the inner circle, but were found on the rosin itself.

103. Metallic plates of various shapes, and under various circumstances were exposed with a view of causing condensation upon them; it was not found possible however either to cause the formation of aqueous dew, or crystalline deposit, except when their temperature was below that of the medium in which they were exposed.

104. At this stage of the inquiry, it becomes important to know, whether along with the rays of light, of heat, and of chemical action, there are not also rays of radiant electricity, emitted by the sun. Almost all operations which disturb the equilibria of light and heat, disturb too that of electricity, and it is well known that, upon this fact, Dr. Hare founds the explanation of the action of certain voltaic arrangements, especially the calorimoter; an explanation, the correctness of which, later researches make more probable. If light, heat and electricity are set in motion by the force of chemical action, and are often found co-existing, there is nothing improbable in meeting them together in the case before us. It is very true, that as yet we have not met with any example of electricity, under what we understand as a radiant form, but that it consists of undulations of an elastic medium, like the undulations of light and heat, is not to be doubted. The experi-

ments of Nobili give proof of an interference, analogous to the interference of the rays of light, which has served so well to refer the motions of that fluid to the undulations of an elastic medium; the analogies of light and heat are every where kept up, and we look with confidence that they will be extended hereafter to electricity.

105. " Quelle imposante découverte ne serait-ce pas, si l'on parvenait á déduire de la lumiére rayonnante, les propriétés par lesquelles les èlectricités neutralizées se signalent." (Berzelius T. de Ch. T. 1, p. 45.) The tendency of the experiments here communicated, is to show that certain substances, conductors of electricity, have the faculty of depriving glass of that power by which it causes the condensation of vapours upon it when exposed to the sun; that deposition will not take place on metallic surfaces, but that certain vitreous and resinous bodies, interfere in no manner with the process. The inference appears inevitable, that electricity brought into play in some unusual manner, is the cause of the phenomenon.

106. By the action of the solar ray, electricity of high tension *can* be developed. A copper electrical condenser was taken, the plates of which were about one-fortieth of an inch apart, and six inches in diameter; there was nothing more in their construction than is met with in the usual arrangement. Another condenser was also provided, which was connected with a gold leaf electrometer, the plates being one inch in diameter, and separated from each other by a very thin coat of gum lac varnish. Trials were repeatedly made to discover whether the apparatus was trustworthy. It is a common complaint against instruments intended to indicate low charges of electricity, that they furnish evidence of an accumulation when none has been communicated; it is necessary therefore to examine each instrument by strict tests, to be certain that this charge cannot be preferred against it. Having obtained this preliminary evidence in a satisfactory manner, and having decided the effectual goodness of the instruments in other particulars, the following trial was made. The six inch condenser was exposed to the sun-beam for one hour, on a clear bright day; the charged plate was then parted, and applied to the one inch condenser; the plates of this being parted, a small but perfectly distinct electric action was obtained. This experiment is not however devoid of sources of error, as from the friction occasioned by touching the plate of one condenser with the plate of the other, or the heating action of the ray, which might cause currents of air to brush over it, but it was found, by purposely rubbing one plate of the condenser on the other, that no charge of electricity could be produced, even if the friction were continued during some time; and on maintaining the temperature of the condenser at the same point to which it was brought by the sunbeam, in order to produce like currents of air, no divergence whatever of the gold leaves was produced.

107. When the tension of electricity is high, one of the most delicate methods of detecting its presence, is by the light it emits in vacuo; the excitation caused by the tremulous motion of a column of mercury in a barometer tube, is rendered visible by the bright light it gives out, when no other method could discover it. On this principle, attempts were made to detect electrical action in the sunbeam, by exposing metallic plates of large dimensions to the ray, and causing any electricity they might gather, to give out light in a vacuum; these trials did not prove satisfactory.

108. It has been stated in another part of these papers, that the cloud which rises from phosphorus when slowly oxydating, is endowed with great mobility; for certain purposes it makes a very good electroscope. When a

contain liquids at *c d* without leaking. Into this torricellian vacuum, I passed a piece of camphor, and exposed the arrangement to the sun; having filled the interstice with water, it was found to have crystals on the aphelion side, there being a ring of them as at *e e*, Fig. 19, all round the tube. This fact being observed, the water was poured out and a solution of sulphate of copper and ammonia introduced; on examination it was found, that on the side nearest the sun no crystals were to be seen, but on the other side there was a dense bed of them, extending exactly half way round the tube, and very much resembling the shape of Fig. 20. A yellow liquid, the bichromate of potassa was next introduced, a result to all appearance exactly like the former was again produced, but having observed that the thickness of the media had a very sensible effect, apparently due to their becoming warm, and not casting off their caloric with sufficient rapidity by radiation, I made an alteration in the arrangement, by interposing between the torricelian vacuum and the light, a trough capable of containing the different solutions. This trough being filled with solution of bichromate of potassa, and the ray tested that it could not blacken chloride of silver; in about one hour the tube presented the following appearance:—there were some pretty large crystals which extended round the tube, as at *a* Fig. 21, which, on the aphelion side, suddenly mounted up, forming a kind of hyperbola, on the anterior semi-circumference not a solitary one was to be seen. The trough being now filled with sulphate of copper and ammonia, the arrangement of the crystals was found to be in every respect like the former.

113. Supposing that this result might in some measure depend on the ray having been subjected to reflexion, before passing through the trough, I repeated the trials, when the sun's altitude was small enough to permit the rays to pass without requiring reflexion, yet still the same results were uniformly obtained; so that whether the chemical or the calorific rays were stopped, crystallization took place on the aphelion side of the tube.

114. May it not therefore be, that this attractive force originates wherever the colorific ray impinges on a surface; it does not necessarily follow from the phenomena, that any peculiar class of rays are emitted by the sun, which bring about this action, but if there are such, it is a question of interest to find what is the reason that good conductors of electricity, render their action nugatory.

(TO BE CONTINUED.)

TO THE COMMITTEE ON PUBLICATIONS.

Formulæ for the announcement of the principal phases of the Annular Eclipse of the Sun, Sept. 18th, 1838. By E. O. KENDALL. *Communicated by* SEARS C. WALKER.

GENTLEMEN:

I send you for insertion in the Journal of the Franklin Institute, the following formulæ, computed after the method of Woolhouse, for facilitating the computation of the principal phases of the annular Eclipse of the Sun of September 18th, 1838, for Philadelphia and places not far distant. The part which relates to limits of latitude will be found particularly useful in enabling observers to select convenient places for observation.

$$\begin{aligned}\cos.\ \omega_{,,} = &+ 1.43404\\ &- [0.14000]\ \sin.\ l\\ &+ [0.04096]\ \cos.\ l\ \cos.\ (\lambda + 211^{\circ}\ 16'5)\end{aligned}$$

$$\begin{aligned} \cos.\ \omega &= +\ 40.98359 \\ &\quad -\ [1.57169]\ \sin.\ l \\ &\quad +\ [1.40297]\ \cos.\ l\ \cos.\ (\lambda + 230^{\circ}\ 45.'5) \end{aligned}$$

$$\begin{aligned} \cos.\ \omega'' &= +\ 1.59753 \\ &\quad -\ [0.18039]\ \sin.\ l \\ &\quad +\ [9.94995]\ \cos.\ l\ \cos.\ (\lambda + 248^{\circ}\ 12.'5) \end{aligned}$$

$$\begin{aligned} t_{,,} &= +\ 9^{h}\ \ 26^{m}\ \ 52^{s}.3 \\ &\quad -\ [3.70767]\ \sin.\ \omega_{,,} \\ &\quad -\ [3.74730]\ \sin.\ l \\ &\quad -\ [3.84836]\ \cos.\ l\ \cos,\ (\lambda + 216^{\circ}\ 42.'6) \end{aligned}$$

$$\begin{aligned} t_{,} &= +\ 9^{h}\ \ 0^{m}\ \ 55^{s}.4 \\ &\quad -\ [2.25147]\ \sin. \\ &\quad -\ [3.65312]\ \sin.\ l \\ &\quad -\ [3.82359]\ \cos.\ l\ \cos.\ (\lambda + 235^{\circ}\ 48.'9) \end{aligned}$$

$$\begin{aligned} t &= +\ 9^{h}\ \ 0^{m}\ \ 55^{s}.4 \\ &\quad -\ [-\ \infty\ ] \\ &\quad -\ [3.65312]\ \sin.\ l \\ &\quad -\ [3.82359]\ \cos.\ l\ \cos.\ (\lambda + 235^{\circ}\ 48.'9) \end{aligned}$$

$$\begin{aligned} t' &= +\ 9^{h}\ \ 0^{m}\ \ 55^{s}.4 \\ &\quad +\ [2.25147]\ \sin. \\ &\quad -\ [3.65312]\ \sin.\ l \\ &\quad -\ [3.82359]\ \cos.\ l\ \cos.\ (\lambda + 235^{\circ}\ 48.'9) \end{aligned}$$

$$\begin{aligned} t'' &= +\ 8^{h}\ \ 56^{m}\ \ 7.^{s}9 \\ &\quad +\ [3.61381]\ \sin.\ \omega'' \\ &\quad -\ [3.56251]\ \sin.\ l \\ &\quad -\ [3.79452]\ \cos.\ l\ \cos.\ (\lambda + 252^{\circ}\ 56.'2) \end{aligned}$$

$$\begin{aligned} \cos.\ (N + l_{,}) &= -\ [0.22011]\ \sec.\ (\lambda + 230^{\circ}\ 45.'5)\ \cos.\ N \\ \cos.\ (N + l^{\circ}) &= -\ [0.20964]\ \sec.\ (\lambda + 230^{\circ}\ 45.'5)\ \cos.\ N \\ \cos.\ (N + l') &= -\ [0.19891]\ \sec.\ (\lambda + 230^{\circ}\ 45.'5)\ \cos.\ N \end{aligned}$$

$$\begin{aligned} \tan.\ N &= +\ [0.16872]\ \sec.\ (\lambda + 230^{\circ}\ 45.'5) \\ L &= +\ [1.85612]\ \cos.\ \omega \end{aligned}$$

Where

$t_{,,}$ = Greenwich mean time of beginning.
$t_{,}$ = " " formation of the Ring.
t = " " nearest approach of Centres.
t' = " " rupture of the Ring.
t'' = " " end.
λ = Longitude from Greenwich + East, — West.
l = Geocentric latitude of place of observation.
$l_{,}$ = " " Northern limit of the annular eclipse for longitude λ.
l° = Geocentric latitude of central eclipse for longitude λ.
l' = " " Southern limit of the annular eclipse for longitude λ.

L = least distance of centres in seconds of arc of great circle.

l, l° and l' may be reduced to geographical latitudes by adding the angle of the vertical, which is 11′ nearly.

These equations have been computed for Philadelphia, for which place only they are rigorously correct. The sum of the Semi-diameters has been diminished 5″ for irradiation.

If we assume 6″, as the probable error in the difference of the sun's and moon's declination taken from the Nautical Almanac, we shall have 30 geographical miles, nearly, as the probable error in the limits of latitude resulting from the above formulæ.

The following table contains the times of the principal phases, correct for Philadelphia, and approximate for several other places, with the limits for the annular phase expressed in geographical latitudes.

	Baltimore.	Harrisburg.	Haverford.
Geographical latitude,	39° 17′ 13″	40° 16′	40° 1′ 12″
Longitude from Greenwich,	76 37 50	76 50	75 19 0
Beginning, mean time,	3h 6m 36s.2	3h 3m 24s.0	3h 11m 35s.5
Formation of Ring, mean time,	4 24 44. 0	4 21 54. 2	4 29 33. 3
Nearest approach, "	4 27 38. 8	4 24 36. 7	4 31 59. 6
Least distance of centres,	14.″4	29.″7	41.″1
Rupture of the Ring, mean time,	4h 30m 33s.6	4h 27m 19s.2	4h 34m 25s.9
End, "	5 40 4. 4	5 37 16. 8	5 43 56. 9
N. limit of ann. phase for this long	42° 43′ 12″	42° 51′ 42″	41° 52′ 12″
Lat. of central eclipse "	38 30 42	38 37 48	37 46 42
S. limit of ann. phase "	35 0 18	35 6 54	34 20 54

	New York.	Philadelphia.	Washington.
Geographical latitude,	40° 42′ 40″	39° 56′ 59″	38° 52′ 54″
Longitude from Greenwich,	74 1 8	75 10 59	77 1 48
Beginning, mean time,	31h 6m 34s.1	3h 12m 25s.4	3h 5m 28s.4
Formation of the Ring, mean time,	4 35 5. 3	4 30 18. 7	4 23 43. 7
Nearest approach, "	4 36 17. 9	4 32 44. 7	4 26 42. 0
Least distance of Centres,	65.″6	41.″3	2.″5
Rupture of the Ring, mean time,	4h 37m 30s.5	4h 35m 10s.7	4h 29m 40.s3
End, "	5 47 47. 7	5 44 38. 0	5 39 14. 5
N. limit of ann. phase for this long.	41° 5′ 24″	41° 47′ 6″	42° 59′ 24″
Lat. of Central Eclipse, "	37 6 30	37 42 24	38 44 24
S. limit of ann. phase, "	33 45 6	34 16 0	35 12 48

Franklin Institute.

Quarterly Meeting.

The Fifty-fourth Quarterly Meeting of the Institute was held at their Hall on Thursday, July 20th, 1837.

Mr. Joseph M. Trueman, was called to the Chair.

Isaac B. Garrigues, Recording Secretary.

The minutes of the last meeting were read and approved.

Donations of Books were received from:

The Hon. Secretary of War U. S., Washington City, D. C.—the Institute of Civil Engineers—the Royal Society—Zoological Society—Society for the encouragement of Arts, Manufactures and Commerce; James D. Forbes, Esqr., and Petty Vaughan, Esq., London; David Stevenson, Esq., Edinburgh; the Royal Irish Academy, Dublin; Prof. Alex. D. Bache, Wm. Duane, Jr. Esq., Peter A. Brown, Esq., Messrs. Carey & Hart, and Mr. John Thomason, of Philadelphia.

Donations to the Cabinets were received from Franklin Peale, Esq., of Philadelphia, Abraham Morrison, Esq., of Cambria Co., Penna., and Capt. Jeffries of the Steamboat Ohio.

The Actuary laid on the tables the periodicals received in exchange for the Journal of the Institute since the last meeting.

The quarterly report of the Board of Managers, was read and accepted.

On motion, the resolution contained in the report of the Board of Managers and by them recommended, relative to Meteorology was adopted.

The Treasurer presented his quarterly report of the finances of the Institute, which was read and approved.

On motion, the reports were referred for publication.

[Extract from the Minutes.]

JOSEPH M. TRUMAN, *Chairman.*

ISAAC B. GARRIGUES, *Rec. Sec.*

Fifty-fourth Quarterly Report of the Board of Managers of the Franklin Institute.

At the time of the last quarterly meeting, the public operations of the Institute in the interesting department of Instruction were just brought to a close for the season. Since that time the summer recess has continued as usual, and the Committee on Instruction have been giving consideration to arrangements for opening the approaching session with renewed means of usefulness.

In its numerous other departments, the silent labours of the Institute have been attended with a sufficient measure of success to encourage us to new and increased exertions: among these the Journal continues to claim and receive primary consideration and care; the number just issued completes the 19th volume, and it is believed will be found to contain an amount of valuable and interesting original and selected matter, fully sufficient to maintain its standing among scientific periodicals. Owing to circumstances not within the control of the Committee on Publications, the last few numbers have been delayed beyond the usual time; but it is hoped that the publication of the future numbers may be accelerated so as to restore their issue to the proper periods.

The accompanying report from the Committee on Sciences and the Arts, affords evidence of unimpaired activity and usefulness on their part, and of the high estimation in which their decisions are held as well by government as by individuals.

In accordance with the regulation adopted at their foundation, the monthly conversation meetings will be suspended during the months of July and August; but it is believed that the members are now entering into the spirit of these meetings with a zeal that will ensure to them, when they shall be renewed in the autumn, a continuance of the interest by which they have been characterised during the past quarter.

At the request of a number of the members both of the Institute and of the Mechanics' and Tradesmen's Exchange Co. arrangements have recently been made to open the reading room of the Institute daily (Sundays excepted) from 10 o'clock A. M., until 2 P. M., so as to provide for our members a convenient exchange for the transaction of business and the deposit of letters.

The superintendence of the Exchange room has been referred to a standing committee of eight members, consisting of Messrs. Jno. S. Warner, John Struthers, Isaac B. Garrigues, Andrew M. Eastwick, John Gilder, Joseph M. Truman, Jesse Williamson, and Briton Corlies.

On report of this Committee, the following rules have been adopted for the regulation of the Exchange:

1st. The hour of high change shall be 11½ o'clock, A. M.

2d. Members shall have the privilege of introducing to the Exchange room strangers, who shall then have free access to the room during one month. All visiters must be presented to the Actuary, and their names, occupations, and residences be entered in a book appropriated for the purpose, with the signature of the member by whom they are introduced.

3d. Any member mutilating the books, newspapers, or other articles in the room, or removing them from the room without permission from the proper authority, shall be liable to such penalty as the Board of Managers may exact.

4th. No member, whose annual contribution for the current year is unpaid shall be entitled to the privileges of the Exchange room.

The Board have delegated to the Committee on the new Hall the duty of carrying into effect the resolutions adopted at the late special meeting of the Society. That Committee are proceeding in the performance of the duty assigned them and the measure thus far promises the desired success. The certificates of loan in small amounts have been engraved on steel and printed in a style at once creditable to good taste, and secure from the danger of spurious imitations. These certificates may be confidently recommended to the members of the Institute and the public, as possessing a security not surpassed by the issues of any other institution.

Connected with this subject, and in order to provide the funds for the redemption of the certificates, the Board has authorized the Actuary to receive subscriptions of five dollars, and weekly deposits of sums from one dollar upwards, to accumulate at interest until the same amounts to twenty-five dollars, when a transferable certificate in the Six per cent. Loan, redeemable in 1856, will be issued to the depositor. It is believed that there are many members of the Institute who have hitherto been deterred from subscribing to the Loan by the uncertainty of the time when they might be called on to pay the instalments, and the plan now proposed will put it in the power of every one to contribute to the erection of a suitable building for the accommodation of the Institute, and at the same time secure a good Interest from his savings in a perfectly safe investment. The resolution of the Managers on this subject is herewith communicated and the Board request that an extensive circulation and recommendation of the plan be given by the members.

Resolved, That in order to provide permanently for the redemption of the evidences of debt issued pursuant to the authority given by the special meeting of the Institute, held July 9th, 1837, and to meet the payment to the Grand Lodge hereafter to fall due, the Actuary be authorized to receive subscriptions to the Six per cent. Loan of the Institute, redeemable in 1856, on the following plan—the first payment on the subscription to be five dollars and the subscribers to make weekly payments thereafter of one dollar or upwards, to accumulate at interest until the same amount to twenty-five dollars, when the Treasurer shall issue, to each subscriber who has that amount to his credit, a transferable certificate of the said Six per cent. Loan.

An application has recently been made to the Board of Managers by the Meteorologist of the Joint Committee of the American Philosophical Society and of the Franklin Institute, asking some action on the part of the Institute, for the purpose of bringing the subject of Meteorology before Congress.

The Board therefore recommend to the Institute the adoption of the following preamble and resolution:

Whereas, a considerable number of members of this Institute, in conjunction with members of the Philosophical Society, have recently devoted much time to the observation of the phenomena accompanying the various conditions of the atmosphere, with a view to ascertain the general laws by which the changes of weather are governed; and the belief is confidently entertained, that a continuance of these observations, and their extension over the widely spread territory of the United States, will enable us to determine many general facts of a highly important and useful character. Therefore,

Resolved, That the Joint Committee of the American Philosophical Society and the Franklin Institute be and are hereby authorized, on the part of this Institute, to memorialize Congress for the purpose of obtaining national aid in furtherance of this interesting object.

Monthly Conversation Meeting.

The Tenth and last Monthly Conversation Meeting of the season was held at the Hall of the Institute, on Thursday, June 22d, 1837.

The meeting was numerously attended, and the number of interesting objects was unusually large; among them was a specimen of caoutchoucine procured from the works of Messrs. Enderby by our fellow member Prof. A. D. Bache. Professor Hare offered some interesting remarks upon this curious liquid, he describes it as an extremely light and volatile substance, highly inflammable, and producing intense cold by its rapid evaporation. The vapour possesses great specific gravity and can be poured like water from one vessel into another. This experiment was shown by the professor, who at the same time cautioned members not to repeat it without great care, as the inflammable and explosive nature of the vapour renders it very dangerous to handle. It is obtained by distillation of caoutchouc and is used as a solvent of caoutchouc in the preparation of water proof cordage.

Dr. J. K. Mitchell presented a small model of a bedstead admirably calculated to promote the ease of invalids.

Mr. S. D. Breed exhibited a large gum elastic hose intended as a substitute for a slide in fitting a gas chandelier.

An easy chair, invented by ——— Clark, of New York, ingeniously contrived to encourage indolence, was exhibited by Mr. T. Roberts.

This piece of furniture comprises, under an elegant exterior, all the properties of an arm chair with or without rockers, a cradle capable of extension to correspond with the increasing dimensions of the occupant, a bed in which the back and head may be readily inclined at any desired angle, and the feet or either of them elevated, depressed or extended at pleasure; in addition to which it could be instantly converted into a four wheeled carriage, for the transportation of its *passenger* from place to place.

A Polygraph, or machine for writing several letters simultaneously, was exhibited and described by Mr. Franklin Peale, who presented the instrument to the Institute to be deposited in their Cabinet, Mr. Peale stated that it was of a small size intended for use during a journey, with two pens, but that others of larger size for two or more pens, had been in ordinary use for many years, that it was the joint invention of John J. Hawkins at present of London, and the late Charles Wilson Peale, by whom it was patented about the year 1805, and that his father always used it from that time during the remainder of his life, a period of about twenty years, the venerable Thomas Jefferson having also used it during the same length of time.

Mr. Peale also exhibited an improved metallic pen, from the same prolific source in mechanics, Mr. J. J. Hawkins of London, which appears to have combined in this instrument, all that has hitherto been found deficient in its use.

The point or nib was represented to be made of a native alloy of Osmium and Iridium, possessing a degree of hardness equal to the ruby, soldered to a plate of Palladium, a metal remarkable for its elasticity, forming the body of the pen, these metals being insoluble in the acids of inks, and the point from its extreme hardness, not being liable to wear, give to these pens an unknown degree of durability, which has been tested by four years constant use without any perceptible change.

A Portrait Lathe, made for the Mint of the United States, was also exhibited by Mr. Peale, an instrument of highly perfected construction by "Contamin" of Paris, intended to copy and reduce medals or other designs in relief, upon steel, ivory, or other material, with specimens of the work executed upon it, shewing a faithfulness of copy that was truly admirable. Whilst describing this lathe Mr. Peale took occasion to present to the notice of the meeting, a beautiful miniature bust in ivory of Franklin, the property of Mr. Joseph Saxton, executed upon a lathe invented by John J. Hawkins, by Chevendon of London, and stated at the same time that this instrument was now brought to such perfection, by the latter gentleman, as to copy full length figures in a reduced size, with perfect accuracy, and of an admirable degree of finish.

Progress of Practical and Theoretical Mechanics and Chemistry.

Gilding Copper, Brass, &c.

A patent was granted to George Richards Elkington, of Birmingham, England, for his invention of an improved method of gilding copper, brass, and other metals or alloys of metals.—[Sealed 24th June, 1836.]

This invention consists in gilding copper, brass, and other metals, or alloys of metals, by means of potash or soda combined with carbonic acid, and with a solution of gold.

The patentee states that in order that his invention may be fully understood, he will describe the process, which has been found fully to answer the purpose; the articles operated upon having a very beautiful appearance, and in most instances have been considered to be gilded in a superior manner to those articles submitted to the gilding process where quicksilver is used.

The process of gilding by the aid of quicksilver being well known, and in general practice, no description of it will be necessary. I will, therefore, says the Patentee, first describe the preparation of the materials, and then explain the process of using the same.

Dissolve five ounces, troy weight, of fine gold in fifty-two ounces, avoirdupois weight, of nitro-muriatic acid of the following proportions, videlicet: twenty-one ounces of nitric acid, pur. of 1.45 specific gravity; seventeen ounces of muriatic acid, pur. of 1.15 specific gravity. and fourteen ounces of distilled water.

The gold being put into the mixture of acids and water, they are to be heated in a glass, or other convenient vessel, till the gold is dissolved; and it is usual to continue the application of heat after this is effected, and until a reddish, or yellowish, vapour ceases to rise.

The clear liquid is to be carefully poured off from any sediment which generally appears, and results from a small portion of silver, which is generally found in alloy with the gold. The clear liquid is to be placed in a suitable vessel of stone; pottery ware is preferred. Add to the solution of gold four gallons of distilled water, and twenty pounds of bicarbonate of potash of the best quality; let the whole boil moderately for two hours, the mixture will then be ready for use.

The liquid being thus prepared, and as in practice it is difficult to keep the liquid hot in stone-ware vessels when many articles are being dipped, it has been found advantageous to transfer the liquid to a cast-iron vessel, which it is necessary to keep very clean.

The articles to be gilded having been first perfectly cleaned from scale or grease, they are to be suspended on wires, conveniently for a workman to dip them in the liquid, which is kept boiling. The time required for gilding any particular article will depend on circumstances; partly on the quantity of gold remaining in the liquid, and partly on the size and weight of the article; but a little practice will readily produce sufficient judgment to the workman.

Supposing the articles desired to be gilded to be brass or copper buttons, or small articles for gilt toys, or ornaments of dress, such as ear-rings or bracelets, a considerable number of which may be strung on a hoop, or bended piece of copper or brass wire, and dipped into the vessel containing the boiling liquid above described, and moved therein, the requisite gilding will be generally obtained in from a few seconds to a minute; this is when the liquid is in the condition above described, and depending on the quality of the gilding desired; but if the liquid has been used some time the quantity of gold will be lessened, which will vary the time of operating to produce a given effect, or the colour required, all which will quickly be observed by the workman; and by observing the appearance of the articles from time to time, he will know when the desired object is obtained, though it is desirable to avoid taking the articles out of the liquid as much as possible.

When the operation is completed, the workman perfectly washes the articles so gilded with clean water; they may then be submitted to the usual process of colouring.

If the articles be cast figures of animals, or otherwise of considerable weight, compared with the articles above mentioned, the time required to perform the process will be greater.

In case it is desired to produce what is called a dead appearance, it may be performed by several processes: the one usually employed is to dead the articles in the process of cleaning, as practised by brass-founders and other trades: it is produced by an acid, prepared for that purpose, sold by the makers under the term "*deading aquafortis*," which is well understood.

It may also be produced by a weak solution of nitrate of mercury, applied to the articles previous to the gilding process, as is practised in the process of gilding with mercury, previous to spreading the amalgam, but generally a much weaker solution; or the articles having been gilded may be dipped in a solution of nitrate of mercury, and submitted to heat to expel the same, as is practised in the usual process of gilding.

It is desirable to remark that much of the beauty of the result depends on the well cleaning of the articles, and it is better to clean them by the ordinary processes, and at once pass them into the liquid to be gilded.

The Patentee says, he has always employed the usual means for cleaning the articles from scales, and other impurities, which are commonly resorted to, in working of the metals for other purposes, when the surfaces are required to be freed from scales or other impurities; and remarks, that great care should be observed in purchasing the articles above described, of the best description.

In conclusion, the Patentee says, "I have described only the using of bicarbonate of potash, which I believe to be the best material for the purpose; and I would remark, that soda in a state of carbonate may be employed, but so far as my experience goes, not with such advantage as potash in a state of bi-carbonate, as above described.

"Having now described the nature of my invention, and the manner of performing the same, I would have it understood, that although, in order to give the best information in my power, I have stated exact quantities of the articles employed, I do not confine myself thereto, nor do I claim any process for cleaning or deading; but what I claim as the improved process of gilding, secured by the present Letters Patent, is the gilding copper, brass, or other metals or alloys of metals, by means of potash or soda, combined with carbonic acid, and with a solution of gold, as above described."

Newton's Jour.

Patented Improvement in the material for Fining or Clarifying Liquids.

A patent was granted to John Dyer, of London, for an improvement in the material used for fining or clarifying liquids.—[Sealed 22d October, 1835.]

The subject of this patent is the production of a material which, when mixed with wine, beer, and some other spirituous and fermented liquors, shall act chemically upon those liquors, for the purpose of dispelling or precipitating such matters held in solution as may have caused the liquors to become turbid.

The material proposed is principally a compound of albumen, carbon, and lime. White of egg and animal blood are suggested as the most convenient matters from whence the purifying materials may be obtained, which are to be evaporated to dryness, and then pulverised.

These matters may be dried either in an open or a close vessel, but a close retort is preferred; and a sand or water bath of 110 deg. Fahr. is proposed as the evaporating medium.

Beside white of eggs and blood, bones may be employed, which should be from young animals, and when perfectly dry are to be pulverised. Clay, or other similar earthy substance, may be used, and also pulverised animal charcoal.

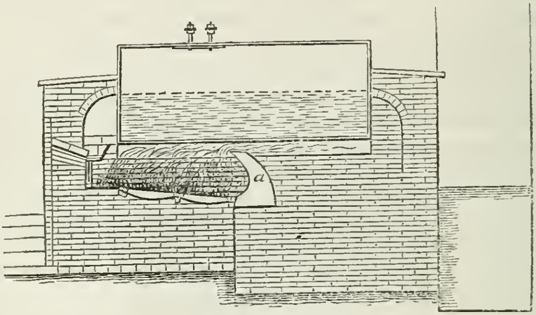
The powders thus produced, may be combined in the proportions of three parts of white of egg to one of blood. About the measure of a cubic inch of this compound, mixed with a small quantity of water, is to be introduced into a pipe of red port wine and stirred up with it, and after a short time the wine will become fine and bright.

A compound of three parts blood, one part bones, and one part yellow marl; or of two parts blood, two parts animal charcoal, and one part bone, all reduced to a fine powder, and mixed with water, may be applied in a similar way to purify white wines, cider, beer, and other liquors.

The Patentee says, in conclusion, he is aware that blood has been used, and also animal charcoal, for clarifying saccharine liquor; he, therefore, does not claim those, unless combined with the other substances; but that which he particularly claims, is "producing the whites of eggs in the state of powder, and compounding them as above described." Ibid.

Description of an improvement in Furnaces for Steam Engine Boilers, &c.

By John Hopkins, of London.



This is a patented improvement. The drawing represents the furnace of a steam-engine boiler, having my improved construction of the fire-bridge made of fire-brick, as shown at *a*, the nature and construction of which will readily be understood by an inspection of the drawing; and it will be seen that the fire-bridge, *a*, in place of rising up and merely intercepting the flames and heated vapours, and causing them to rise upwards and impinge more effectually on the boiler, as has been the construction of ordinary fire-bridges of steam-engines, and other boiler furnaces; the bridge, *a*, shown in the drawing, curves forward, and in addition to causing the flames and heated air to pass upwards, and bring them more closely to the boiler, those flames from the ignited fuel nearest the bridge are turned back, and have a tendency to pass towards the fire-door, and hence the vapours arising from the partially ignited fuel must mix with and be consumed by such flames so directed by the curved construction of the flue, as above described; and it will be evident that in marine and some other construction of boilers the fire-bridge, in place of fire-brick, may be formed of metal, and hollow, for the water of the boiler to pass into and become heated therein, either by pipes connected therewith, or by the bridge being connected to the sides of the boiler, where the construction is such as to have the furnace within; all which will be readily understood by an engineer accustomed to the arranging and constructing of boilers. And it will be evident that, although I have only shown and described furnaces for steam-engine boilers, the invention is equally applicable to the furnaces of other boilers wherein bridges of the ordinary construction have been heretofore used. Repertory of Inven.

Manufacture of Liquid and Paste Blacking.

Specification of a patent granted to William Bryant, and Edward James, of Plymouth, England, for improvements in the manufacture of Liquid and Paste Blacking.—Sealed January 27, 1837.

For the liquid blacking, take India-rubber oil (which consists of eighteen ounces of India-rubber, or a little more or less, according to its quality, dissolved by heat in nine pounds of rape oil;) fine ivory black, sixty pounds; molasses, forty-five pounds; gum-arabic, finely powdered and dissolved in vinegar, one pound; vinegar (No. 24,) twenty gallons; the above to be well mixed and ground in a mill until perfectly smooth, when add sulphuric acid twelve pounds, in small quantities at a time, stirring it briskly for half an hour. Allow it to remain fourteen days, stirring it half an hour every day, then add gum-arabic, finely powdered, three pounds; repeat the stirring for half an hour every day during fourteen days longer, and then it will be fit for use. And for the paste blacking, take India-rubber oil (which consists of eighteen ounces of India-rubber, or a little more or less, according to its quality, dissolved by heat in nine pounds of rape oil;) fine ivory black, sixty pounds; molasses, forty-five pounds; and gum-arabic, finely powdered and dissolved in twelve pounds of vinegar (No. 24.) The above to be mixed and ground in a mill until perfectly smooth, and then add, in small quantities at a time, sulphuric acid twelve pounds, stirring it briskly whilst adding the acid, and for half an hour after the whole is mixed; in seven days it will be fit for use. Ibid.

Improvement in Bread Making.

Specification of the Patent granted to John Whiting, M. D., of Kennington, England, for an improvement in preparing certain Farinaceous Food. —Sealed May 3, 1836.

To form seven pounds of wheaten flour or meal into bread, mix from 350 to 500 grains of the carbonate of soda with about two pints and three-quarters of pure water (the quantity of the alkali may be made to vary within such limits as the baker finds it suit best, and depending on the degree of lightness required.) Mix with three-quarters of a pint of water, in a separate vessel, so much of pure muriatic acid as will neutralize the quantity of the carbonate of soda employed, the quantity of the said acid varying according to the known specific gravity of the acid, and the quantity of soda in the carbonate, which are subjects familiar to chemists. From about 420 to 560 grains of the acid, as met with in commerce, I have found in practice to be required for 350 grains of carbonate of soda; and I would remark, that as bakers are not usually acquainted with chemistry, in order to their adjusting the proportions of the muriatic acid and the alkali, they must depend on some one who is possessed of chemical knowledge. Let the flour be divided into two equal portions; to one portion thrown into a wide earthenware pan or trough, add the solution of the soda gradually, well stirring and beating the mixture with a large wooden spoon, or other convenient instrument, so as to form a uniform batter, free from all lumps. Scrape down into the batter all pieces from the sides of the pan and spoon before the mixing is finished. Upon this batter throw the other portion of the flour, and while briskly stirring them together from the bottom, pour in gradually the diluted acid, then let the dough be formed, and while in a rough state, let it be thrown on the board, and lightly kneaded with a biscuit brake or rolling-pin for a minute or two, doubling and rolling it until it becomes blended and quite uniform and light, but this process must not be continued long. When this is accomplished the dough may be lightly moulded with dry flour, and baked in loaves distinct from each other, and not very large (from a half a pound to a pound and a half weight of dough each.) The bread in some ovens is found to turn out best when baked under tins, in the form called, in London, Coburg or Coronation loaves. The oven should be hot enough to raise the dough quickly, but not so hot as to bind the crust too soon. The bread requires to be well soaked (as it is technically called by bakers,) it being apt to retain too much moisture if it be not sufficiently long in the oven. The process of mixing should be conducted in a cool place, and the water used should be as cold as can be procured, especially in hot weather. Common salt may be added, sufficient to flavour the bread. The quantity of common salt formed by the ingredients used, is about 280 grains, when 350 grains of the aforesaid carbonate of soda are employed, and a little addition of common salt, about half or three-quarters of an ounce, which is to be dissolved in the diluted acid, will generally improve the taste of the bread. The quantity of water above mentioned will be found to correspond with half a pint to one pound of flour, but this must vary with the strength of the flour, and I have found that a rather soft dough is better than that which is stiff. Great care must be taken in the mixing in all cases to secure a perfect union of the acid and alkali, otherwise the bread will be discoloured.

I would remark, that soda and carbonic acid in their other chemical combinations, as the common crystallized carbonate (called sometimes subcarbonate,) or the dried carbonate, or the true bicarbonate, may be employed for the same purpose, care being taken to obtain the extraction of a sufficient quantity of gas, and to form a neutral mixture of the acid and alkali that is to produce common salt.

When eggs or milk, and butter, sugar, and spices, are to be used in making different kinds of light cakes, the same may be mixed with one portion of the flour before the alkali is added to it, and the dough made as above described, and when biscuits are to be formed, the dough may be made as above described, only it must be rendered stiffer by the addition of flour and by pressure, in the manner resorted to in preparing them in the ordinary way.

The flour of rye, and that of barley and oats, when mixed with some of wheat, may be made into bread by the same process, and potatoes (which must on no account exceed one-third by weight of the quantity of flour intended to be made into bread, the other two-thirds being of wheaten flour,) and rice may also be used with wheaten flour, though the bread will be generally considered best if made wholly of wheaten flour. Ibid.

Increased production of Iron.

The quantity of iron made in this country, during the year 1836, has been estimated at the enormous quantity of *a million tons*; the average price of pig-iron, for the year, being about 7*l.* per ton, and that of bar-iron, about 11*l.* per ton. The make of this year, doubtless, far exceeded that of ordinary years, in consequence of the extraordinary impetus given to the iron trade, owing to railway speculations, and other causes; but if we consider the average quantity to be only eight hundred thousand tons per annum, which will probably not be far from the truth, the vast amount of wealth thus created by the iron trade alone, must be well calculated to excite surprise and admiration.

Of this large production, we may consider about one hundred and fifty thousand tons to be annually exported to foreign countries, chiefly in a wrought or manufactured state; a large proportion, within the last year or two, being in the form of rails, castings, &c., for the construction of railways. One of our principal exports of iron, during the last year, was to the United States, to be employed in the great projected lines of railway in that country; and a considerable quantity of rails and castings were imported into Russia from England, to be employed in forming the first line of railway which has yet been executed in that country.

The removal of local obstacles to the carrying on of a manufacture of such immense importance, must be regarded as an object of great interest; while any improvement in the manufacture itself, must be considered in the light of a national benefit. We have reason to believe that objects thus important, have lately been effected by the successful application of the carbonaceous variety of coal, termed "anthracite," to the smelting of iron —an experiment has, for some time past, occupied the attention of Mr. Crane, of the Yniscedwyn iron works, near Swansea, and of which some notice has lately appeared in our columns.

The great coal-field of South Wales is well-known to be one of the most extensive coal districts in Great Britain, and one in which the deposits of this valuable mineral are comparatively little worked; while the rich strata of iron ore, associated with the coal, have rendered it one of the principal seats of our iron manufacture. These great local advantages are, however, in a great measure, confined to the eastern extremity of this coal-field, where the bituminous coal prevails, while a large portion, and more especially towards the western extremity, contains only "stone-coal," or "anthracite," which will not bear coking, and is, therefore, inapplicable to the manufacture of iron. Thus, all the great iron works are situated in the vicinity of Pontypool and Merthyr Tydvil, while none exist farther west. To remove this obstacle to manufacturing industry, both in a large part of South Wales, and other districts similarly situated, has been the object of Mr. Crane's experiments; and in this, from the information we have received, he has completely succeeded, chiefly by the use of the "hot blast"—the most important modern auxiliary which the art of metallurgy has received.

Independently of the local advantages thus obtained, there is, we understand, a positive benefit derived from the use of this new fuel, as the iron thus made resembles in its qualities the charcoal iron of the continent, which in some respects is of much finer quality than that manufactured in this country, where coke is always used. The great similarity in the chemical composition of anthracite and vegetable charcoal, renders such a result highly probable; and should this be the case, we must congratulate the inventor on having discovered a substitute for a species of fuel which our country is unable to furnish in sufficient abundance for the manufacture of certain qualities of iron.

We are not yet in possession of the details of the improvements alluded to, which, however, we hope shortly to be enabled to lay more fully before our readers—but the subject is of so much interest, as connected with one of our most important manufactures, that we have thus briefly adverted to it without waiting to receive further information. The increasing competition of foreign countries is a circumstance to which we have often alluded, and we, therefore, hail with pleasure every improvement which, like the present, promises to render more available the vast mineral wealth with which Great Britain has been so liberally gifted by nature, and which requires so great an amount not only of actual labour, but of energy and talent, for its continued and beneficial extraction. Mining Journal.

On the Composition of Bitumens. By M. BOUSINGAULT.

The bitumens which are so abundantly spread over the face of the globe, and whose uses are every day becoming more varied and extensive, have hitherto been little examined. If we except the labours of M. de Saussure upon the naphtha of Amiano, we are nearly in complete ignorance concerning the intimate nature of these bituminous compounds.

It is to this insufficiency of the data supplied by chemistry that we must attribute the confusion into which most mineralogists have fallen in their attempts to classify the different bitumens. A systematic place can easily be assigned to naphtha, idrialine, and mellilite or honey-stone, but when we come to petroleum we get involved in difficulties; this substance usually met with in a liquid state, now becomes viscid, and successively presents all possible degrees of consistence, till we arrive at asphaltum, which is solid and brittle. We are generally led to admit that the bitumens owe their fluidity to naphtha; but the results of the present investigation show that there is no ground for this supposition. The attention of the author was first directed to the viscid bitumen of the *Department du Bas-Rhin.* After describing the method in which the bituminous sand is treated, he gives a rapid sketch of the locality of bitumens; and shows that the immense masses of mineral pitch which are found on the banks of the River Magdalena, at Payta, upon the coast of Peru, have a geological position precisely similar to that in which we find bituminous impregnated sands in Europe; that is to say, in formations which we must refer to the supercretaceous group. The only contradictory fact opposed to this conclusion is that recorded by M. de Humboldt, when he states, that at La Punta d'Araya, in the Gulf of Cariaco, he saw petroleum issuing from mica-slate.

The bitumen of Bechelbronn is viscid, of a very deep brown colour. The uses to which it is applied have procured for it the name of mineral grease.

Heated to 120°, this bitumen does not yield any product; but, on distilling it with water, an oily principle is procured, which is volatile, of a pale yellow colour, in which analysis shows there is nothing else than carbon and hydrogen. As this carburetted hydrogen appears to constitute the principal liquid of bitumens, the author designates it by the appellation *Petroline.*

Petroline boils at 280° of the mercurial thermometer; its odour is that of bitumen; at 21° its weight is 0.891, even at a cold of 12° it does not lose its fluidity; alcohol dissolves it in small quantity, and it is much more soluble in ether. It contains carbon 0.885, hydrogen 0.115, and is consequently isomeric with the essential oils of turpentine, lemon, and copaiba.

By using the methods employed by M. Dumas, it is found that the density of the vapour of petroline is equal to 9.415, whilst calculation indicates 9.533. This is precisely double the weight of the vapour of the essence of turpentine. If we admit that four volumes of vapour constitute an atom of petroline, its atomic composition will be as follows:—

Carbon 80 atoms, . .	3060.8
Hydrogen, 64 atoms, .	400.0
	3460.8

Independent of the petroline, there exists in the bitumen a black solid substance, which is absolutely insoluble in alcohol, and soluble in ether. M. Boussingault names this product *Asphaltine*, because it forms the base of that species of mineral which mineralogists describe under the name of asphaltum.

Asphaltine may be procured by subjecting bitumen purified by ether to the continued action of a temperature of 240° or 250°. Asphaltine is solid, very black and brilliant, its fracture conchoidal; at a temperature of 300° it becomes soft and elastic, and is decomposed before the blowpipe. It contains Carbon, 0.753; Hydrogen, 0.099; Oxygen, 0.148, a composition which is represented by the formulary $C.^{80}. H.^{64}. 0^{6}.$ which indicates that asphaltine is the result of the oxidation of petroline.

The bitumen of Bechelbronn, purified by ether, seems to be nothing more than a mixture of petroline and asphaltine. It contains Carbon, 0.870; Hydrogen, 0.112; Oxygen, 0.018.

On the whole, then, it would appear that glutinous bitumens are mixtures, probably in all proportions, of two substances, which we can isolate, and which have each a difinite composition. One of these principles is solid and fixed, and in its nature approaches to asphalt; the other liquid, oily, and volatile, resembles petroleum in some of its properties. Hence we may readily conceive how the consistence of bitumens varies in a way we may call indefinite; its degree of fluidity being regulated by the relative proportion of the mixture of its ingredients.

The analogy which exists between the asphaltine and the asphaltum of mineralogists, led me, says the author, to inquire if this analogy is maintained in their composition; I have submitted the asphaltum of Coxitambo (Peru,) which may be considered as the type of the species, to analyses. This asphaltum has a fracture which is eminently conchoidal, with a high degree of lustre. The weight is 1.68; it is decomposed before the blowpipe, and when burned leaves 0.0016 residuum. It contains Carbon, 0.750; Hydrogen, 0.095; Oxygen, 0.155. Edin. New Philos. Jour.

Watch Statistics.

Mr. Dent, (Arnold and Dent,) in his illustrations at a lecture on the construction of watches and chronometers, given by him at the Royal Institution on the 7th ult., laid before the meeting the dissection of a detached lever watch (compensation-balance,) every part was separated and displayed, but grouped in one of six larger divisions to which it belonged.

Each part had been previously examined, and its distinct constituent pieces counted by the lecturer; the surprising result of this enumeration was exhibited in a table, of which we lay a copy before our readers. In addition, will be found the number of kinds of artificers concerned in the operations necessary for the construction of a good watch. When to these are added the amount of previous operations which the materials constituting each piece must undergo, *before* it comes into the hand of the watch-artificer, a glimpse may be obtained of the extensive and numerous changes of form and value which "raw material" receives in its progress, from the mine to so refined a manufacture as a finished watch.

No. of Parts.	No. of Pieces.	Trades employed.
1. Pillars	4	1
2. Frame	4	1
3. Cock and Potence	2	1
4. Barrel and Arbor	3	1
5. Going-Fuzee	14	2
6. Wheels	4	1
7. Pinions	4	2
8. Stop-Stud	1	1
9. Stop and Spring	3	1
10. Click and Ratchet	3	1
11. Motion	16	2
12. Jewels (5 holes)	28	2
13. Cap	3	2
14. Dial	5	3
15. Index	1	1
16. Escapement	13	3
17. Compensation-Balance	9	1
18. Case	3	1
19. Pendant	2	1
20. Case-Joint	6	1
21. Case-Spring, &c.	4	2
22. Main-Spring	1	2
23. Chain	826	3
24. Hands	3	1
25. Glass	1	1
Total of Pieces	992	
Engine-Turner		1
Engraver		1
Gilder		1
Examiner		1
Total of kinds of Artificers employed		43

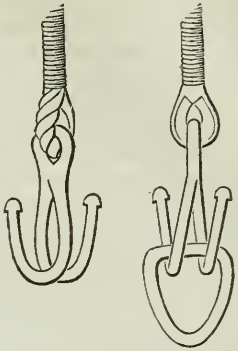
Mag. Pop. Sci.

Double Hook.

The thanks of the Society were voted to Lieutenant G. Beadon, R. N., for his Double Hook; one of which has been placed in the Society's Repository.

It appears that single hooks are in use in the navy, for the purpose of hoisting packages and other articles on board ship, and that, to prevent accidents, a general order is given that no article is to be hoisted on board unless the hook has been first *moused*, that is, unless a piece of yarn has been passed round the end of the hook and the opposite part of the shank, so as to prevent the article from slipping off. A scrupulous adherence to the above order was the occasion of a package of dollars being lost in transferring it from a boat to the deck of a frigate on the coast of Peru, in which Lieutenant Beadon was at that time serving; for, just as the hook had been passed under the cording of the package, and before it had been moused, a roll of the ship raised it out of the boat. The package might, with perfect safety, have been hoisted on board; but the order about mousing the hook not having been complied with, the package remained dangling at the end of the rope, and, as the ship rolled back again, was struck by the gunwale of the boat and thrown off the hook into the sea.

To avoid such accidents, Lieutenant Beadon has invented a double hook, as shown in the annexed figure, the two bends of which point in opposite directions, and each bend has a double set, somewhat like a fish-hook. A hook of this description will, apparently, require no mousing, and is easily both made fast and disengaged.



Trans. Soc. of Arts.

Progress of Physical Science.

Phenomena of Petrifaction.

The petrifaction of wood, and more especially its silicification, still continues to present obscure problems to the botanist and chemist. The first step towards their solution will probably be made by carefully examining vegetables in different stages of petrifaction; and with this view Mr. Stokes has procured several specimens of wood, partly mineralized and partly not. Among these is a piece found in an ancient Roman aqueduct in Westphalia, in which some portions are converted into spindle-shaped bodies consisting of carbonate of lime: while the rest of the wood remains in a comparatively unchanged state. The same author has pointed out cases both of siliceous and calcareous fossils where the lapidifying process must have commenced at a number of separate points, so as to produce spherical, or fusiform, petrifactions, independent of each other, in which the woody structure is apparent, while in the intervening spaces the wood has decayed having, after removal, been replaced by mineral matter. In some petrifactions, the most perishable, in others the most durable, portions of plants are preserved, variations which doubtless depend on the time when the mineral matter was supplied. If introduced immediately on the first commencement of decomposition, then the most destructible parts are lapidified, while the more durable do not waste away till afterwards, when the supply has failed, and so never become petrified. The converse of these circumstances gives rise to exactly opposite results. As to the manner in which the minutest pores and fibres discoverable by the microscope, even the spiral vessels themselves can be turned into stone, or have their forms faithfully represented by inorganic matter, no satisfactory explanation has ever yet been offered. In considering, however, this question, you will do well to consult the important suggestion which a celebrated chemist, our late lamented Secretary, Dr. Turner, has thrown out on the application of chemistry to geology. He reminds us that whenever the decomposition of an organic body has begun, the elements into which it is resolved are set free in a state peculiarly adapting them to enter into new chemical combinations. They are in what is technically termed a nascent state, the constituent molecules being probably of extreme smallness and in a fluid or gaseous form, ready to obey the slightest impulse of chemical affinity, so that if the water percolating a stratum be charged with mineral ingredients, and come in contact with elements thus newly set free, a mutual action takes place, and new combinations result, in the course of which solid particles are precipitated so as to occupy the place left vacant by the decomposed organic matter. In a word, all the phænomena attendant on slow putrefaction must be studied whenever we attempt to reason on the conversion of fossil bodies into stone; and in regard to silicification, Dr. Turner has shown how great a quantity of silex is set free as often as felspar decomposes, and how abundantly siliceous matter may be imparted from this source alone to running water throughout the globe.

As I have mentioned the name of Dr. Turner, I cannot pass on without an expression of sorrow for the untimely death of that amiable and distinguished philosopher. Mr. Whewell and Mr. Murchison alluded in most feeling terms this morning at the General Meeting to this melancholy event, which is too recent and too painful to myself and others to allow me now to dwell longer upon it.

Before quitting the subject of vegetable petrifactions, I ought to mention a memoir just published, by Mr. H. R. Goppert, Professor of Botany at Breslau, "On the various Conditions in which Fossil Plants are found, and on the Process of Lapidification."* He has instituted a series of most curious experiments, and his success in producing imitations of fossil petrifactions has been very remarkable. I have only space to allude to one or two examples. He placed recent ferns between soft layers of clay, dried these in the shade, and then slowly and gradually heated them, till they were red hot. The result was the production of so perfect a counterpart of fossil plants as might have deceived an experienced geologist. According to the different degress of heat applied, the plants were obtained in a brown or perfectly carbonized condition, and sometimes, but more rarely, they were in a black shining state, adhering closely to the layer of clay. If the red heat was sustained until all the organic matter was burnt up, only an impression of the plant remained.

The same chemist steeped plants in a moderately strong solution of sulphate of iron, and left them immersed in it for several days until they were thoroughly soaked in the liquid. They were then dried and kept heated until they would no longer shrink in volume, and until every trace of organic matter had disappeared. On cooling them he found that the oxide formed by this process had taken the form of the plants. Professor Goppert then took fine vertical slices of the Scotch fir, *Pinus Sylvestris*, and treated them in the same way; and so well were they preserved, that, after heating, the dotted vessels so peculiar to this family of plants were distinctly visible. A variety of other experiments were made by steeping animal and vegetable substances in siliceous, calcareous, and metallic solutions, and all tended to prove that the mineralization of organic bodies can be carried much farther in a short time than had been previously supposed.

These experiments seem to open a new field of inquiry, and will, I trust, soon be repeated in this country. In endeavoring, however, to verify them, the greatest caution will be required, or we may easily be deceived. We must ascertain, for example, with certainty, that every particle of animal or of vegetable matter is driven off before we attempt to determine the full extent to which mineralization may have proceeded. Professor Goppert is doubtless aware that coniferous wood may be burnt and reduced to charcoal, and after having been kept for some time at a red heat, will continue to exhibit, on being cooled, the disks, or reticulated structure, to which he alludes. If, therefore, some small particles of carbon remain in the midst of the oxide of iron, such portions may retain traces of the vessels peculiar to coniferous wood; and an observer not on his guard, might infer that the same structure was preserved throughout the mass.

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* Poggendorff, Annalen der Physik und Chemie vol. xxxviii. part 4. Leipsic, 1836.

In my last address, I alluded to Mr. Lonsdale's detection of vast numbers of microscopic corallines and minute shells in the substance of the white chalk of various counties in England, where this rock had not been suspected of consisting of recognisable organic bodies. I cannot deny myself the pleasure of mentioning the still more singular and unexpected facts brought to light during the last year, by Professor Ehrenberg of Berlin, respecting the origin of tripoli. I need scarcely remind you, that tripoli is a rock of homogeneous appearance, very fragile and usually fissile, almost entirely formed of flint, and which was called polir-schiefer, or polishing slate, by Werner, being used in the arts for polishing stones or metals. There have been many speculations in regard to its origin, but it was a favorite theory of some geologists that it was a siliceous shale hardened by heat. The celebrated tripoli of Bilin in Bohemia consists of siliceous grains united together without any visible cement,and is so abundant that one stratum is no less than fourteen feet thick. After a minute examination of this as well as of the tripoli from Planitz in Saxony, and another variety from Santa Fiora in Tuscany, and one from the Isle of France, Ehrenberg found that the stone is wholly made up of millions of siliceous cases and skeletons of microscopic animalcules. It is probably known to you, that this distinguished physiologist has devoted many years to the anatomical investigation of the infusoria, and has discovered that their internal structure is often very complicated, that they have a distinct muscular and nervous system, intestines, sexual organs of reproduction, and that some of them are provided with siliceous shells, or cases of pure silex. The forms of these durable shells are very marked and various, but constant in particular genera and species. They are almost inconceivably minute, yet they can be clearly discerned by the aid of a powerful microscope, and the fossil species preserved in tripoli are seen to exhibit, in the family Bacillaria and some others, the same divisions and transverse lines which characterize the shells of living infusoria.

In the Bohemian schist of Bilin, and in that of Planitz in Saxony, both of them tertiary deposits, the species are freshwater, and are all extinct. The tripoli of Cassel appears to be more modern, and the infusoria in that place which are also freshwater, are some of them distinctly identical with living species, and others not. In the tripoli brought from the Isle of France, the cases or shells all belong to well-known recent marine species.

The flinty shells of which we are speaking although hard are very fragile, breaking like glass, are therefore admirably adapted when rubbed for wearing down into a fine powder fit for polishing the surface of metals. It is difficult to convey an idea of their extreme minuteness, but I may state that Ehrenberg estimates that in the Bilin tripoli there are 41,000 millions of individuals of the *Gaillonella distans* in every cubic inch of stone. At every stroke therefore of the polishing stone we crush to pieces several thousands if not myriads of perfect fossils.—*Extracted from the Anniversary address before the Geological Society of London, by the President Charles Lyell, jun. Esq.* 1837. Lond. & Ed. Philos. Mag.

Action of Presence.

The 60th volume of the *Annales de Chimie et de Physique* contains a paper by Pelouze on a new acid, which he calls *nitrosulphuric acid.* Speaking of the salt which this forms with ammonia, he says, "the excessive mobility of the elements of nitrosulphate of ammonia, and the stability which the alkales impart to them, made me think it not impossible that this salt might exhibit phænomena of decomposition of the same class as the singular ones which M. Thenard observed with binoxide of hydrogen. In fact this is the case; many substances which decompose binoxide of hydrogen, without either acquiring or losing anything, also decompose the nitrosulphates. Spongy platina, oxide of silver, metallic silver, powdered charcoal, and oxide of maganese, produce this effect; the two first-mentioned especially, act with extreme rapidity upon the nitrosulphate of ammonia.

"I convinced myself," continues M. Pelouze, "that this remarkable phænomenon was due, as in the case of binoxide of hydrogen, to *an action of presence* (à une action de presence,) and that it never produces more than a mere conversion of nitrosulphate of ammonia into protoxide of azote and sulphate of ammonia. Oxide of silver is not reduced, for if it be washed after having caused it to decompose a large quantity of this salt, it afterwards dissolves in nitric acid without the disengagement of red vapours."

M. Pelouze afterwards remarks that "it would be very difficult to discover the probable cause of these singular phænomena; but from the very circumstance of their being at present inexplicable, they appeared to me the more to merit the attention of chemists. What indeed is more calculated to excite curiosity than to observe a salt, by the mere contact of a body which neither yields anything to nor takes any thing from it, decompose with extreme rapidity into new forms, among which the agent producing these violent actions remains chemically passive.

"Two compounds, namely, binoxide of hydrogen and hydruret of sulphur, are already known as possessing the property of decomposing by the influence of a simple action of presence."—*Ann. de Ch. et de Phys.*, lx. 158.
L. & E. Philos. Mag

Catalysis.—Action of Presence.

In the 61st volume of the *Ann de Ch. et de Phys.*, Berzelius has followed up the remarks of Pelouze *on the action of presence*, and has given to it the name of *Catalysis.* His notice on this subject is entitled, "*Some ideas on a new force acting in the combinations of Organic Compounds.*"

In inorganic nature, new combinations are formed between the different bodies which are present, because these bodies have a greater tendency to combine with each other than with other bodies. Those bodies which have a great affinity for each other, combine together, and reject those for which they have a weaker affinity, and with which they were previously combined; and these also combine together. Until the year 1800, this tendency of bodies to unite was the only one known, except that heat, and in some cases light, could effect their combination. The influence of electricity was of later discovery, but it was soon also discovered that chemical and electrical affinity were the same, and that heat and light acted only by increasing or diminishing these affinities. On proceeding to the study of organic chemistry, very different bodies were obtained from the same crude material by different organs. In animals, this crude material, which is the blood, flows in the uninterrupted vessels, and gives rise to all the different secretions; such as milk, bile, urine, &c., without the presence of any foreign body which could form new combinations.

Kirchoff discovered that starch dissolved in acids diluted with water was converted at a certain temperature into gum, and then into sugar of grapes; and yet there was no combination between the elements of the acid and those of the starch, for no disengagement of gas took place. On treating the solution with bases, all the acid employed was regained; the solution contained only sugar, the weight of which only slightly exceeded that of the starch employed. Some time afterwards, Thenard discovered a new substance, the binoxide of hydrogen, the elements of which are held together by very slight affinity. This substance was not decomposed by acids, but alkalies gave its elements a tendency to separate; slow effervescence occurred with the disengagement of oxygen, and water was formed. It was soon found not only that soluble substances produced this effect, but also that other organic and inorganic bodies, such as manganese, silver, platina, gold, fibrin, &c., acted in the same manner upon it. This decomposition takes place by the mere presence of the foreign body, in consequence of a power which is at present unknown, and without the smallest quantity entering into the new compound, for the most minute researches failed in discovering the slightest alteration in it.

Edmund Davy discovered that if extremely finely divided platina be moistened with alcohol, this on taking fire ignites the platina, and the alcohol, if it contains water, is converted into acetic acid. This led to the grand discovery of Dœbereiner, that spongy platina has the property of firing a current of hydrogen directed upon it. This discovery was soon followed by that of Dulong and Thenard, who found that it was not platina alone that possessed this property, but that other bodies, such as gold, silver, and glass, acted in the same way, but only when they are exposed to a somewhat high temperature; whereas platina, iridium, and other metals which accompany platina, produce this effect even much below the temperature of melting ice.

The analogy was observed which exists between the phenomena of the conversion of sugar into alcohol by the presence of a foreign insoluble body, and that of the decomposition of peroxide of hydrogen into water and oxygen, by the presence of platina, silver, fibrin, and some other equally insoluble bodies. No case analogous to that of the decomposition of binoxide of hydrogen by the presence of the alkalies dissolved in this substance was known, for at this period the analogy of this phenomenon with that of the formation of sugar by means of starch and sulphuric acid was not known. Something similar occurs in one of the hypotheses on the formation of ether. According to this hypothesis, sulphuric acid should combine with one part of the water contained in the alcohol, and thus would form ether; but it could not be explained why other bodies, such as potash, chloride of calcium, quicklime, &c., which have a very great affinity for water, did not produce the same effect. Mitscherlich showed that if alcohol be poured upon sulphuric acid, at a temperature higher than that of boiling water, water and ether are distilled together, and form a mixture the weight of which is precisely equal to that of the alcohol employed. Thus the sulphuric acid does not there act in consequence of its affinity for water; its action is analogous to that of the alkalies on the binoxide of hydrogen; and that of sulphuric acid upon starch in the formation of sugar.

It is then proved that many bodies, both simple and compound, soluble and insoluble, have the property of exerting on other bodies an action which is very different from chemical affinity. By means of this action they produce decompositions in bodies, and form new compounds into the composition of which they do not enter.

This new power, hitherto unknown, is common, both in organic and inorganic nature. I do not believe that it is a power which is entirely independent of the electro-chemical affinities of the substance; I believe, on the contrary, that it is merely a new form of it; but as long as we do not see their connexion and mutual dependence, it will be more convenient to describe it by a separate name. I shall therefore call it *catalytic power.* I shall also call *Catalysis* the decomposition of bodies by this force, in the same way as the decomposition of bodies by chemical affinity is termed analysis.

The following are the questions which occur relative to this catalytic power.

Can this catalytic power produce differences in catalytic products, according to its greater or less intensity?

Can different bodies possessing catalyic power produce different catalytic effects upon the same compound substance?

Can bodies which possess catalytic power exert this action on a great number of different compound substances, or is this action confined to a few substances?

These questions can be answered only by extended researches; it is enough at present to have established the existence of this power by a sufficient number of examples. This power gives rise to numerous applications in organic nature; thus it is only around the eyes of the potatoe that diastase exists; it is by means of catalytic power that diastase, and that starch which is insoluble, is converted into sugar and into jam, which being soluble form the sap that rises in the germ of the potatoe. This evident example of the action of catalytic power in an organic secretion is not probably the only one in the animal and vegetable kingdoms, and it may hereafter be discovered that it is by an action analogous to that of catalytic power that the secretion of such different bodies is produced, all which are supplied by the same matter,—the sap in plants and the blood in animals.

Lon. & Ed. Philos. Mag.

On Solid Carbonic Acid.

Letter from M. Thilorier to the Academie des Sciences of Paris.—"I have the honour of now announcing to the Academy that I have just finished a second memoir upon liquid carbonic acid, in which, after having successfully examined the different parts of this body, its *specific gravity*, which is so variable that, from 32° to 86° Fahr., it successively runs through the whole scale of densities from water to that of ethers; its *dilatability*, which is four times greater than that of air itself; its *pressure* and the *weight* of its vapour; its *capillarity*, and especially its *combressibility*, which is a thousand-fold greater than that of water, I have succeeded in determining in the most exact manner, the uniform and constant law which regulates all these phenomena, which at first view appear altogether independent of each other. The Academy will without doubt learn with interest, that, by means of a very simple apparatus, I have succeeded in producing instantly, and economically, masses of solid carbonic acid weighing an ounce and an ounce and a quarter, and which the experimental chemist may beneficially employ My first experiments on cold, which I have already presented to the Academy, were made by directing a stream of liquid carbonic acid upon the bulb of a thermometer, or on tubes which enclosed the different substances upon which the action of the cold was tried. This method had the serious inconvenience of wasting a great quantity of the liquid, and of leaving some uncertainty upon the maximum of the cold produced. The facility and abundance with which I now obtain the solid carbonic acid has supplied me with a method of experimenting which is infinitely preferable. The bulb of the thermometer having been introduced into the centre of a small mass of solid carbonic acid, at the end of one or two minutes the thermometer became stationary and stood at —194° Fahr. Some drops of ether and of alcohol poured upon the solid mass did not produce any appreciable difference less or more on the temperature. Ether forms a mixture which is half liquid, and of the consistence of melting snow; but alcohol, in combining with solid carbonic acid, congeals, and produces a hard, brilliant, and semi-transparent ice. This freezing of anhydrous alcohol only takes place in the act of mixture; when isolated, as in a silver tube, in the midst of a mass of solid carbonic acid, the alcohol undergoes no change whatever. The mixture of alcohol and solid carbonic acid begins to melt at —185° Fahr, and starting at this point, the temperature does not vary any more. Thus we can obtain from this extreme limit a point as fixed as is supplied by that of melting ice. If, after having formed *a small coppel* of solid carbonic acid, we pour into it three or four drachms of mercury, it is seen to congeal in a few seconds, and to remain in this new condition so long as an atom of solid carbonic acid remains, that is to say, for twenty or thirty minutes when the weight of the coppel is from two to three drachms. I have already said that the addition of ether or alcohol did not augment the real degree of the cold, but, by giving the solid carbonic acid the power of moistening bodies, and of adhering more intimately to their surface, these substances much increase the frigerating effects. A piece of solid carbonic acid, on which some drops of ether or alcohol are poured, becomes capable of congealing fifteen or twenty times its weight of mercury. The promptitude with which it is converted into the solid state, the mass in which it is effected, and which may easily exceed half a pound, and its continuance in this new condition, which may be maintained as long as you like, with the single precaution of placing the metallic mass upon a base of solid carbonic acid, leads me to believe that this method of freezing mercury will henceforward be substituted for all those which have been previously employed."

Edin. New Philos. Jour.

Progress of Civil Engineering.

Architectural Hints.

An idea for forming artificial gardens in confined spaces at the back of houses in town, was once started to me by a friend, which I consider too good to be lost, although it may startle many, at first, as an extravagant absurdity. It was, to have branches of plants and flowers, moulded in earthenware, and painted of their natural colours, but unglazed, and after wards affixed to real stems, planted either in beds or spots. These would produce a lively and cheerful appearance, as seen from the window of a back parlour, and would constitute a perennial garden in full bloom at all seasons, and let the situation be what it might; while, during the summer, pots with real shrubs and plants, might either take the place of, or be intermixed with, the others. Nor, were the practice adopted, and manufactories for the purpose established, would the expense of such artificial plants be very great, at least not for a sufficient quantity to make a decent show, and screen dead walls; because, being intended to be seen only from a distance, they would not require to be nicely finished or coloured, nor to have more pains bestowed on them than would suffice to produce the desired effect, so that they might be manufactured by children. "Still," cries an objector, "it would be mere make-believe." Certainly: so is gilding, or painting in imitation of grained woods; and I admit that it would be supremely foolish for any one to put himself to the expense of such a deception, if he were equally well satisfied with having before his eyes a forlorn looking, dingy yard, styled by courtesy a garden, with a few leafless and stunted plants stuck in it. Nay, after all, whether absurd or not, that something of the sort has already been practised with tolerably good effect, may be learned from the publication I have already quoted, and which deserves to be quoted again. "The Italian taste in chimneys," says the author of *Slight Reminiscences*, "is peculiarly elegant; no matter how mouldering, raw, or staring the house may be, there is always something graceful and redeeming about this generally frightful utility. We stepped out from the show apartments of the Palazzo Brignoli, at Genoa, on a spacious terrace, which constitutes the roof of another part of the building. The chimneys were placed at regular intervals, each having the form of a column, ornamented with trophies, and terminated by a vase, containing an aloe, or something that looked like one. The effect was so good that we believed them to be merely decorative, until our cicerone pointed out to us a light smoke issuing from the centre of the leaves." If, therefore, a deception of that kind was allowable, there would hardly be anything very extravagant in one that would not be quite so much at variance with *vraisemblance.* Arch. Mag.

On an Improved method of Teaching Drawing; by JOHN ROBISON, Esq.

From an article in the appendix to "Lectures on Popular Education, by George Combe."

"It appears to me," Mr. Robison observes, "that every one who can write is capable, with a slight effort, of making every line or mark which is wanted in order to represent any object presented to him. It is not, therefore, the mechanical use of the pen or the pencil which requires to be taught, so much as the art of looking at objects, and *of recognising what we really see.* When the habit of noting the true visual forms of objects has been acquired (which it will soon be, if cultivated under the directions of an intelligent instructor,) the power of delineating the outline will not be long found wanting; the perception of the effects of light and shade may be acquired in the same way, and they will then be rendered on paper by the pupil with a degree of truth which he could not attain by any time or labour spent in copying the drawings of others.

"If a young, or uninstructed, person be required to make a representation of such an object as a common pencil, he will probably proceed to mark on his paper an outline of the actual length and breadth of the pencil, but he will be at a loss to show that it is round and not square; again, he will not be able without consideration, or perhaps explanation, to delineate on paper the different appearances which the pencil assumes when held nearer to or further from the eye; or in positions more and more oblique until nothing be seen but the circular end. A little pains on the part of the instructor would lead a pupil to observe and comprehend all that is required to do this, by making him attend to what he really sees; and the lesson, when once acquired, would be in little danger of being forgotten, although it in fact include the whole doctrines of perspective.

"In forming any institution for teaching drawing *as an useful art*, I should therefore propose that the pupils should, from the very commencement, be exercised in noting and delineating the appearances of a few simple objects, presented to their view at varied distances, heights, and degrees of inclination. A convenient object may be found in a cubical box of wood, fitted to slide on an upright rod or stand, on which it may be fixed at any desired height by a hollow through its axis. If this model be set in front of a pupil, at such a distance that it can be conveniently seen, and its height be made that of his eye, and one of the sides be parallel to his face, then on noting its appearance, he will soon observe that it may be represented by a square outline, parallel to the sides of his paper. If the model be then raised by sliding it up the rod, the pupil will find that a change in the *apparent* form has taken place, and that his outline must include a representation of the bottom, which he will be enabled to give, by combining his present observations with what he learned in studying the changes of position of the pencil in the earlier lessons. He will also find, that the degrees of light falling on the two faces which he now sees are different, and require different shadings from the pencil. In the first case, the single face of the cube which he saw may have been either lighter or darker than the distant back-ground, and in the delineation some shading may have been required on the back-ground, or on the object, according to which appeared darkest to him; but, in this second case, he may have three degrees of light to represent, according to existing circumstances. In the same way, the position of the model may be varied, both in respect to figure and to light; or, if a class be under instruction, the pupils may interchange their places round the object, and each in succession take similar views, and compare the results at the conclusion of each series.

"It is obvious that such a system of instruction may be pursued to a great extent, and with the variations which may be required according to the views of the pupils; and that, even for those who intend to pursue the higher branches of the Fine Arts, a better foundation could hardly be given for enabling them to understand and profit by the examples left by the great masters." Ibid.

Proposed Railways in England.

"The number of proposed railways, including diversions, extensions, and branches, in England and Wales, for which plans have been lodged in the Private Bill Office in the present session, is seventy-five, of which only forty-eight are under the consideration of Parliament; these amount in length to twelve hundred and thirty-three miles, and are estimated at the sum of of nineteen millions three hundred and fifty-two thousand seven hundred and twenty-six pounds,* or fifteen thousand six hundred and ninety-five pounds per mile. The whole length of tunneling is twenty-five miles; and ehe number of bridges, exclusive of viaducts and culverts, two thousand tight hundred and twenty-five, or nearly two and a third per mile. The weight of iron required for the rails, is one hundred and ninety-three thousand five hundred tons; and of stone for the blocks two millions six hundred and seventy thousand tons. The area of land required to be taken, is upwards of fifteen thousand acres; and of felt for the chairs, one hundred and thirty acres. These railways, if carried into execution, would employ at least five thousand men and fifteen hundred horses for three years.† The principal competing lines are the five Brighton, the two Birkenhead and Chester, and the South Union, and Manchester, Cheshire and Staffordshire Railways.

"The proposed railways in England and Wales, abandoned in the present session, are twenty-seven in number, amounting in length to seven hundred and ninety-four miles; the length of tunneling is about eight and three-quarter miles; and the number of bridges one thousand five hundred and ninety-five, or about two per mile." Mining Jour.

Locomotive Engine.

Locomotive engines of very imperfect construction were in use for a long time previously to their introduction on public railways. The Stockton and Darlington Railway is the first on which this master-piece of human skill was introduced as a substitute for horse-power. Many of the engines still used on this line are very ponderous and clumsy, but still they are powerful. The Lord Brougham may be quoted as an example; it is an engine sixteen feet in length, supported on six heavy wheels, each three being connected together by cranks; these wheels are each four feet in diameter, the whole weight is about twelve tons. The two cylinders are placed in front of the chimney, and are each fourteen inches in diameter, the piston of each working vertically, and communicating with the cranks beneath, attached to the wheels.

" On the Manchester and Liverpool Railway, the proportion of the number of engines in daily use, is almost one to every three miles, and about as many more under repair: among those at present in use may be mentioned, the Collier, the Mercury, the Rapid, the Hercules, the Ajax, the Speedwell, and the Thunderer. Some of these engines were built by Messrs. Taylor and Co., of the Vulcan Foundry, near Warrington.

"On the Dublin and Kingstown Railway, where the passenger traffic is very considerable, the trains starting from either end of the line, almost twenty times a day, the number of engines is nine; they are severally called the Manchester, the Britannia, the Vauxhall, the Kingstown, the Dublin, the Comet, the Victoria, and the Stanley. Three of these were built by Messrs. Sharp and Roberts, of Manchester, and have eleven-inch cylinders, and sixteen-inch stroke; and the remainder by Mr. Forrester, of the Vauxhall Foundry: the weight of these engines is from about nine to ten tons; many of them are of elegant design.

" On the Leicester and Swanington Railway, where the tonnage of coal alone, from Mr. Stephenson's and other collieries, is about 500 tons a-day, the number of engines is seven, viz. the Comet, the Hercules, the Goliah, the Vulcan, the Atlas, the Sampson, and the Liverpool.

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* This sum is exclusive of four of the proposed Brighton lines, one of the Southern lines from Manchester, one of the Birkenhead and Chester lines, the Great Western, Southampton, North Midland, and Eastern Counties' Diversions; and also the line from Gilingham to Exeter.

† For the earthworks alone.

"The Atlas and Vulcan, each with six wheels, are two of the most powerful engines at present in use, on any line of railway, having cylinders of sixteen inches diameter, and twenty-inch stroke; they are built for heavy loads and slow speeds.

"The Hercules, Goliah, and Samson, have each fourteen-inch cylinders, and eighteen-inch stroke; and, lastly, the Comet and Liverpool, twelve-inch cylinders, and fourteen-inch stroke.

"The valuable work on locomotive engines by the Chev. de Pambour, contains the best possible information with regard to the construction of the locomotive engine." Ibid.

Mineral Resources of Belgium.

The following is extracted from the "Almanack Belge," a publication containing much useful statistical information, and which affords the best summary we have yet seen of the mineral resources of that country:—

"Belgium possesses beds of coal and iron mines, more valuable than the silver veins of Peru, or the gold of Brazil. The coal-beds of Belgium are superior to any on the European continent. In the basin of Mons, for instance, we find from a hundred and ten to a hundred and twenty strata, or layers of coal, disposed one above another, all workable, and all wrought. The four principal collieries of Mons, Marimont, Liege, and Charleroi yield annually 3,200,000 tons of coal. The whole produce of the French coal-pits, in 1834, did not exceed 2,500,000 tons. Nor is belgic industry less striking as regards iron than it is with respect to coal. It is remarkable, and somewhat inexplicable, that, to this day, no attempt for the fabrication of iron after the English fashion—that is, by extracting it from the iron-stone or ore (by means of coal or coke)—has succeeded in France, though many districts abound in natural advantages for the purpose. In Belgium, on the contrary, the undertaking has been accompanied with the most marked success. Seven years ago (in 1830.) some capitalists at Charleroi erected a furnace, where they treated the mineral of the district with the coal which likewise exists there. On the first trial, an article was produced which was greedily bought up by the Parisian iron-founders. From that time till this, establishment has followed establishment so rapidly, that, in the district of Charleroi alone, there are, at this day, twenty-five coke-furnaces in action, producing annually 75,000 tons of metal, without reckoning furnaces where wood is employed. In 1834, the iron produce of all France, from the combustible mineral as in Belgium, amounted only to 47,000 tons; the French castings in the same year, of every kind, amounted in the whole to 269,000 tons. So rich in iron and coal, Belgium could not fail to have flourishing manufactures; the woolen stuffs of Verviers, and the flaxen goods of Flanders, are familiar to the world.

"It is known that the French manufacturers of linen, woolen, and other soft goods, have declared the impossibility of competing with the manufacturers of Verviers and Ghent, on account of the greater excellence and abundance of the landed produce, lint, flax, &c. of Belgium; and it is also known, that the iron-workers of France have expressed similar opinions, on the ground of the greater fertility of Belgium in coal. Thousands of French operatives, it is said, have been reduced to beggary from these causes. These remarks are confirmed by the conduct of the French government, which in consequence of the national inferiority in these respects, has judged it impolitic to unite France and Belgium by the strong bond of a commercial association. By this conduct, however, a chance has been left of the accession of Belgium in self-defence to the Prussian commercial league, which is decidedly hostile in its tendency to the interests of France.

"The Belgians follow out their manufacturing and mining pursuits with a boldness and energy most honorable to the national character. Six hundred feet was not long ago reckoned an extraordinary depth to push a mine down into the earth; recently, the Belgian miners have penetrated above thirteen hundred feet; they wrestle fearlessly with subterranean torrents, and with steam-engines raise their waters from their hidden sources. With the lamp of Davy, they defy the treacherous air-damp, which has involved so many in instantaneous destruction. Not less active and industrious are those among the labouring classes of Belgium, whose lot it is to toil in the eye of day.

"While the French are building senseless monuments, the Belgians are busily raising structures for manufacturing industry such as fabrics for the reception of furnaces, plating or boring machines, or forges. One capitalist of Belgium has been known to erect, for the convenience of his own individual business, more buildings than the French artillery destroyed at Antwerp. The establishment of Searing, near Liege, extends, in regular buildings, for a quarter of a league (three quarters of an English mile.) On the side opposite the Meuse are situated the mines, from which issue the mountains of coal, to be converted into coke. Next to these are the great round towers, sixty feet high, and fortified with iron, from the mouths of which flames are continually darted as from a crater. One of these furnaces has been in action for six years, and during that time there have been thrown into it, each day, nearly one hundred thousand killogrammes of materials. The numerous other buildings attached to this establishment are filled with steam-engines, forges, refining places, &c. Almost ere the manufactured iron be cold, it is delivered over to expert artisans, well provided with every species of instrument, who make from it machines and tools of all possible kinds, and, above all, rails for railways, which may be regarded as a sort of steam-horses, surpassing all the Arabian barbs that ever cropped the scanty herbage of the desert. The whole neighborhood of this establishment is traversed by railroads and canals. The workmen at Searing are three thousand in number.

"The boldness of modern industry, and the colossal range of its operations, exact on the part of the men who direct these, an expansion of views and a rapidity of comprehension and decision similar to what is required in a general; and, not unfrequently, to carry the parallel farther, a degree of courage is demanded on the part of the workmen similar to that which has often ennobled the common business of the soldier. We allude to the occasional bursting of furnaces and other accidents liable to occur both above ground and in the mines, at which times a degree of intrepidity and nobleness of spirit has been repeatedly evinced by common workmen, surpassing, from the superiority of the object in view, all the exploits that were ever performed in the 'imminent deadly breach' of war. Many instances of this kind might be related of the workmen of Belgium.

"A nation, cultivating industrious pursuits with so much sagacity and intelligence, so completely absorbed, in fact, in active business, might be expected to avail themselves with ardour of the prospects recently opened up by railroads. On the first of May, 1834, a law to the following effect was promulgated by the Belgian government:—

"'Article 1. There shall be established a system, or chain, of railways having Mallines for a centre point, and stretching to the eastward, toward the Prussian frontier by Louvain, Liege, and Verviers--to the north as far as Antwerp--to the west as far as Ostend, by Termond, Ghent, and Bruges-and to the south by Brussels, and towards the frontiers of France through the Hainault.'

"'2. The railways shall be executed at the public charge, and under the direction of the Government.'

"Thus while France is resting in shameful inactivity, in accordance with the ridiculous principle that it is not proper for a Government to interfere with the course of national industry, the little kingdom of Belgium is marching straightforward in the path of prosperity, without embarrassing itself with any prejudicial question of the sort—this line of railroads, crossing to all the cardinal points of the compass, being at this hour in the course of execution. The line from Antwerp to Brussels is completed, and has been traveled for several months. The revenue derived from it has exceeded the highest expectations. With the exception of the line from Brussels to the French frontier, the construction of which must depend on an understanding between the two countries, the whole of the projected Belgian railways will be completed within two years. It is supposed the eastern, western and northern lines, the aggregate length of which is 222 English miles, will be finished at an expense of forty-five millions of francs (1,875,000*l.* sterling.) The proportionate expense per mile, must, of course, vary according to the difficulties, or the facilities of the ground. That portion of the line which extends between Brussels and Antwerp, a distance of thirty-three miles, passes over ground naturally level, and cost 4350*l.* a mile. More inaccessible districts will cost much more than this, as has been said; but it may be questioned whether any part of the Belgian railways will approach near to the expense of the line between Paris and St. Germains; which is estimated at above 24,000*l.* sterling per mile. The abundance of iron in Belgium, will render all public works of this kind comparatively cheap.

"In addition to the 222 miles of railway first projected, nearly 100 miles additional have been subsequently resolved upon, in order to perfect the internal intercourse of the country. When these plans are all completed, the industry of the people will then have every facility in their way that art can afford them; and, as they possess immense internal resources, instead of being a plaything in the hands of every nation that may choose to go to war, Belgium must become a rich and important state. That its inhabitants will also be amongst the happiest on earth, may be pronounced, from the attention which they pay to that inestimable and indispensible possession—education." Ibid.

On the Expansibility of the different kinds of Stone.

Mr. Alex. J. Adie, Civil Engineer, has performed an extensive series of experiments upon different kinds of stone, as well as upon iron and upon brick, porcelain, and other artificial substances. The instrument employed was a pyrometer of a simple construction, capable of determining quantities not greater than $\frac{1}{30000}$ of an inch. The length of the substances generally employed was 23 inches. The general result of these experiments is, that the ordinary building materials of stone expand but very little differently from cast-iron, and that, consequently, the mixture of those materials in edifices is not injurious to their durability. The experiments from which the expansibility of the substances was numerically determined, were made between the limits of ordinary atmospheric temperature and that of 212°; steam being introduced for that purpose between the double casing of the instrument.

The following results were obtained for the fractional expansion of the length, for a change of temperature of 180° Fahr.

Table of the expansion of stone, &c.

		Decimal of length for 180° Fahr.
1.	Roman cement,	.0014349
2.	Sicilian white marble,	.00110411
3.	Carrara marble,	.0006539
4.	Sandstone from the Liver Rock of Craigleith Quarry,	.0011743
5.	Cast-iron from a rod cut from a bar cast 2 inches square	.00114676
6.	Cast-iron from a rod cast half an inch square,	.001102166
7.	Slate from Penrhyn Quarry, Wales,	.0010376
8.	Peterhead red granite,	.0008968
9.	Arbroath pavement,	.0008985
10.	Caithness pavement,	.0008947
11.	Greenstone from Rutho,	.0008089
12.	Aberdeen gray granite,	.00078943
13.	Best stock brick,	.0005502
14.	Fire brick,	.0004928
15.	Stalk of a Dutch tobacco pipe,	.0004573
16.	Round rod of Wedgewood ware (11 inches long)	.00045294
17.	Black marble from Galway, Ireland,	.00044519

We shall insert Mr. Adie's paper in full as soon as we can find space for it.

P. C.

Mechanics' Register.

Cloth of Glass.

M. Dubus Bonnel of Lille has discovered a method of making a cloth of glass, which is perfectly flexible, and may be applied to the hangings of rooms and other purposes. This cloth of glass is extremely beautiful; and, from the manner in which it reflects the light, it surpasses in brilliancy every thing that has ever been attempted with silk, even when combined with gold and silver. Some specimens of this new manufacture have been exhibited in the Passage de l'Opera in Paris; and the Queen of the French was so much pleased with them, that she ordered a golden medal to be sent to the inventor. The following passage is extracted from a French paper: —"When we figure to ourselves an apartment decorated with cloth of glass, and resplendent with lights, we must be convinced that it will equal in brilliancy all that it is possible for the imagination to conceive; it will realise, in a word, the wonders of the enchanted palaces of the *Arabian Tales.* The lights flashing from the polished surface of the glass, to which any colour or shade may be given, will make the room have the appearance of an apartment of pearls, mother-of-pearl, or diamonds, or composed of garnets, sapphires, topazes, rubies, emeralds, amethysts, &c., or, in short, of all these precious stones united and combined in a thousand ways, and formed into stars, rosettes, bouquets, garlands, festoons, and graceful undulations, varied almost to infinity."—(*L'Echo du Monde Savant*, &c., No. 58, Feb. 15, 1837.) Arch. Mag.

New Locomotive Invention.

Colonel Prince Jasper Bieloselsky-Bielozersky, an officer in the Russian service, has just obtained from the Emperor a ten year's privilege for a new invention, which he denominates a *sledge-road.* The novelty of the principle consists in affixing the wheels to the road instead of to the carriage, the effect of which is said to be a great diminution in the labour of draught. The sledges are proposed to be worked by horses, but there seems no reason why, if the principle should really be found to answer, the superior power of steam should not be made use of. The inventor is sanguine in his expectations of the benefit of his discovery, which, among other recommendations, possesses that of economy. The expense of constructing a sledge-road of the length of a Russian verst would, he calculates, at St. Petersburgh, amount to about 10,350 rubles, which is about £645 per English mile. One of his sledges, which are of a peculiar shape, was manufactured by Clarke, of St. Petersburgh, for 220 rubles. or £9 3*s.* 4*d.* Lon. Mech. Mag.

Locomotive power.

In giving an account of the opening of a portion of the railway, we omitted to mention, as we had intended, that one of the locomotive engines, the Goliah, (George Mann, engineer,) brought down that morning from Kirkhouse, sixty-three wagons of coals in one train—each wagon weighing (with coals) about four tons and a quarter! With this immense load (267 tons) she traveled the distance, twelve miles, in less than forty minutes!—*Carlisle Jour.*

Mining Journal.

It would have been well to have mentioned the grade of the road over which thi train passed. G

Expansive Force of Steam.

A circumstance occurred the other day, rather of a singular nature, which strongly illustrates the powerful effects of steam. A strong stone bottle, half filled with water, and tightly corked, was placed by a servant-girl in an oven and forgotten. The water, of course, began to be converted into steam by the heat of the oven, which burst the bottle, and was so expansive as to drive the oven-door, which was of cast-iron, from its hinges against the kitchen wall, with such violence, that it was broken into several pieces. The oven itself, though of considerable weight, was carried from its seat, blew out both the kitchen windows, and tore down the fire-place. Several children were playing in the kitchen, but they fortunately escaped injury.

Mining Jour.

LUNAR OCCULTATIONS FOR PHILADELPHIA, SEPTEMBER 1837.						Angles reckoned to the right or westward round the circle, as seen in an inverting telescope. ☞ For direct vision add 180° ☜	
Day.	H'r.	Min.	Phase	Star's name.	Mag.	from Moon's North point.	from Moon's Vertex.
1	23	42	Im.	Venus' Centre,		7	336
2	0	52	Em.			267	252
13	7	44	Im.	ψ' Aquarii,	5.6,	163	120
13	8	31	Em.			256	220
17	8	57	Im.	π Arietis,	5,	148	94
17	9	46	Em.			271	218
17	12	18	Im.	ζ^2 Arietis,	6,	137	88
17	13	30	Em.			291	254

Meteorological Observations for May, 1837.

Moon.	Days.	Therm.		Barometer.		Wind.		Water fallen in rain.	State of the weather, and Remarks.
		Sun rise.	2 P.M.	Sun rise.	2 P.M.	Direction.	Force.		
				Inches	Inches			Inches.	
	1	43°	46°	29.80	29.80	NW.	Brisk.		Cloudy—clear.
	2	31	56	30.16	30.10	W.	do.		Clear—do.
⊗	3	42	73	29.66	29.66	SW.	Blustering	.11	Cloudy—do.—thunder showers.
	4	40	70	85	90	W.	Moderate.		Clear—do.
	5	50	76	60	60	E.W.	Brisk.		Cloudy—do.
	6	62	81	50	45	SW.	do	.8	Cloudy—do.—shower.
	7	44	62	93	93	NW.	Moderate		Clear—do.
	8	44	63	93	95	E	Calm.		Cloudy—do.
	9	40	69	30.04	30.04	W.	Moderate.		Lightly cloudy—do. do.
	10	54	71	29.70	29.70	W.	do.	.15	Drizzle.—clear.—rain in night.
	11	50	70	90	93	E.W.	do.		Clear—do.
☾	12	50	76	93	30.00	E,	Brisk.		Cloudy—flying clouds.
	13	56	76	30 00	00	SE.S.	Moderate.		Foggy—flying clouds.
	14	60	82	29.90	29.85	SW.	do.		Fog—clear.
	15	58	70	80	74	E.	Brisk.	1.23	Clear—cloudy—rain.
	16	56	69	50	50	E.SE.	Moderate	.88	Rain—cloudy—showers.
	17	58	70	60	64	S.SW.	Brisk.		Cloudy—clear.
	18	50	67	75	75	SE.	do.	.40	Fog—cloudy—thunder shower.
	19	60	73	64	64	S.	Moderate		Cloudy—lightly cloudy.
☉	20	52	67	64	74	W.	Brisk		Cloudy—clear.
	21	53	74	30.00	30.00	W.	Moderate.		Clear—clear.
	22	60	70	00	29.80	SW.	do.	.18	Clear—do.—showers.
	23	48	67	29 90	30.00	W.	do.		Clear—do.
	24	50	54	30 05	10	E	do.		Cloudy—rain.
	25	51	58	29.80	29 70	NE.	do.		Rain—thunder—rain.
☽	26	51	64	50	60	NE.	do.	2.00	Cloudy—do.
	27	50	74	75	70	W.	do.		Clear—do.
	28	54	68	30 00	30.10	NE.W.	do.		Partially cloudy—clear.
	29	48	76	5	3	E W.	do.		Clear—do.
	30	56	82	26.95	23.94	NW.S.	do.		Lightly cloudy.
	31	62	80	95	92	SW.	Brisk.		Fog—Clear.
	Mean	51.26	69 61	2 985	293 3			5.03	

	Thermometer.		Barometer.	
Maximum height during the month	82. on 14th 30th.	. .	30.16 on	2d.
Minimum do. .	31. on 2d	. .	29.45 on	6th.
Mean do. .	60.43 . .	. .	29..83	

JOURNAL
OF THE
FRANKLIN INSTITUTE
OF THE
State of Pennsylvania,
AND
MECHANICS' REGISTER.
DEVOTED TO
Mechanical and Physical Science,
CIVIL ENGINEERING, THE ARTS AND MANUFACTURES,
AND THE RECORDING OF
AMERICAN AND OTHER PATENTED INVENTIONS.

SEPTEMBER, 1837.

Practical and Theoretical Mechanics and Chemistry.

Notices of various things in existence, or in progress; Extracted from a Letter addressed to the Editor, by Jacob Perkins, Esq., London.

In publishing some communications recently received from our old friend Perkins, we observed that the gentleman, in whose hands they had been placed, had been long detained, and that the information furnished was nearly a twelvemonth old. The letter from which the subjoined extracts are taken, made a part of the papers received, and the Editor hopes that some of the interest felt by him on the matters to which it relates, may still be excited in the minds of his readers.

"You no doubt, in common with many others, have wondered at my long delay in producing some things so confidently promised in my communications to you, and in certain public announcements, made some years since. The particular causes of this delay are too much of a private nature to justify, or warrant, their detail, and I will simply state, therefore, that the extravagant demands made upon me in consequence of the insolvency of persons with whom I have been connected, involved me in law, and that the seal of Chancery was placed even upon my unfinished experiments; this however, is now broken, and since I have been able to resume them, I have compelled the most inveterate of my opponents to acknowledge that I have struck upon some veins in the mines of scientific knowledge well worth exploring. I send you by the bearer, Col. Petival, a model of my new steam-boiler, or rather generator, which I am about to patent both in this country, and in the United States; so far it has realized my most sanguine expectations both as regards economy and safety. The pamphlet which accompanies it [not received] will explain my general views, but the most interesting part of this improvement must remain unpublished until the respective patents are secured.

The paper which I have sent you on the application of the Pneumatic Engine to railway purposes, looks, I must acknowledge, a little *Redhefferish* in some of its parts; it, however, relates actual facts, and will be applied in a manner which will become the subject of patents in the United States.

You still hear from this side of the water accounts of the running, or intended running, of steam carriages upon common roads. I am aware that you have repeatedly expressed an opinion that they could not succeed. This, from the first attempt made, up to the present time, has also been my settled conviction: that is, that they never can so run as to compete with rail-roads. But notwithstanding the numerous failures and abandonments, much money will yet be spent before the project will be given up. There are now two splendid steam tugs to start in a few days, with their attached omnibuses; they will undoubtedly run for a short time, after which they will be withdrawn and soon pass into the shades. This grand concern was patronized by the late celebrated engineer Tellford, who as a constructor of canals, bridges, turnpike roads, &c., &c., has established a fame which will be imperishable. My attachment to him was very great; he was a man to be loved; was extremely liberal in all his views and actions; and to this country his loss is a very heavy one. Had he lived a few years longer, he, I am well assured, would have experienced severe mortification in witnessing the entire failure of his favourite system of steam-coach traveling, which was eventually to supersede the rail-road system. There are, you know, spots upon the sun, and probably upon every other luminary.

I intend shortly to send you a drawing of my ice making machine. It is very simple in its construction, and economical in its operation, so much so that I can make ice from filtered water at a cost of about one shilling per cwt., which does not exceed one-eighth of its market value. This estimate is made under the supposed application of steam-power, manufacturing in the large way. When made by hand its cost will be about a penny per pound.

I have lately obtained a patent for a centrifugal locomotive blower, which gives a much stronger blast than the ordinary revolving blower, when of only one half the size. One of its important features is its giving the blast without noise. The air is drawn in at one side, and is driven out at the other, not at the periphery, as heretofore; every fan is at work throwing the air over the disk, which is equal in diameter to the fans, on the delivery side.—Many blowers which have been put up for steam-boats, cupola furnaces, &c., have been taken down on account of their trumpet-like noise, although they were otherwise very efficient. I intend to try them for a new purpose in steam-boats, namely, to dispense with the huge chimney, and to throw the exhausted air and smoke into the paddle case. Speaking of the paddle case, reminds me of my oblique paddle-wheel, so well understood, and explained by you in your Journal; it has not yet been tried on a large scale, but in all my experiments in the small way it has performed well. I had a boat fitted on one side with the ordinary paddle-wheel, and on the other with my oblique paddles, each exposing an equal surface, and being equal in diameter, the rudder being fixed in a line with the keel; when the wheels were made to revolve, the boat moved in a circle of small radius, showing the great advantage in favour of my wheel. I still hope to live long enough to try it on a large scale on my return to my own country, which I intend shall be at an early day. To you it may appear strange, but it is true notwithstanding, that long as this wheel has been before the public there are very few indeed, even among professed engineers, who understand its true principle.*

The Adelaide Gallery, of which you have frequently heard, continues to attract attention. When I first proposed the establishment of such an exhibition as that which is made there, its success was a subject of much doubt. It is now, however, firmly established. Its average number of visitants is 300 per day, each paying one shilling for admittance; on some occasions there have been 1000; and the visitors are of the most respectable class. My steam gun showers its balls every hour from 12 to 4 o'clock. The gallery opposite to this gun is 150 feet long, and it is frequently filled three or four deep, with intelligent spectators, and although it has been thus in use for three years it still continues to be the lion of the day. Foreigners who visit the gallery not unfrequently avow that the object of their journey from the continent was to see this gun. On a recent occasion just before the last volley was discharged, the atmosphere became suddenly very dark, and many were waiting to see the last shower of balls; the steam was somewhat higher than common, the gun had been recently lengthened, and from the concurrence of these circumstances the velocity of the balls was much increased, and a very remarkable effect was produced. When the balls came into contact with the cast-iron target, a very brilliant group of stars was seen on the plate, each about the size of a marygold, and somewhat resembling it in appearance; light was seen also at the mouth of the gun; and the leaden balls were completely pulverized. What could have produced these effects? was it not the result of the development of electricity by the friction, or from some uninvestigated cause?

The action of a soft iron disk upon hard steel, such as a file, is exhibited four times a day. This has been regularly kept up for three years, yet it has undergone very little wear. I am of opinion, in fact, that if the file had never been held upon it until it had attained its full velocity, there would not have been any loss of metal. I do not know to what extent the combustion of steel by soft iron may have been carried in the United States, but our experiments are so brilliant as to excite the highest admiration, and to induce numbers to repeat their visits to the Gallery. Our disk is a foot in diameter and an eighth of an inch thick. It requires about a three horse power to drive it, and revolves about 6000 times in a minute. It is very accurately fitted up with friction wheels. The blaze of light which rises about 12 inches, perpendicularly, from the point of contact, is so vivid that but few persons can look steadily at it even at noon day. The stream of light is about an inch and a half thick at the distance of a few inches from the point of contact, and at the distance of 7 or 8 feet it spreads out to about 10 inches. The sparks not unfrequently touch the ceiling, which is about 20 feet high; a ring of fire is seen all around the disk, appearing like a band of light about five-eighths of an inch wide. Of what does this light consist? It is manifestly different from that of the sparks, which all fly off in a tangent. In operating with the disk, it never becomes warm; the file, however, has to be held at least two inches from the sharp end, as it becomes highly heated. The whole appearance, in fact, is very interesting, and when fully investigated, I am well convinced that some of the phenomena will be found to depend upon electricity.

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* A gentleman recently from England, an Engineer by profession, and a frequent visitant at the Adelaide Gallery in London, where the model is daily exhibited in operation, denied the possibility of the main thing which characterizes it, when mentioned to him by the editor, yet professed to know every thing about it. See Vol. V, p. 334, of this Journal.

My son Anger is doing well with his patented apparatus for heating by hot water. The system of heating by hot water and steam is being abandoned, and the old apparatus removed from various public buildings to give place to the new. This is the case in the British Museum, where Anger has given great satisfaction. He has fitted up two ships for the North Pole, and orders crowd upon him faster than he can execute them. A thousand reports of explosions, fires, &c., have been invented and propagated, as caused by his system, the whole of which are utterly false, and the intended evil is recoiling upon the interested individuals who have aimed the blow at him. The apparatus has already worked well in various buildings during successive years without losing a drop of water. The filling pipes are regularly examined once or twice a week, when, owing to some invisible leak, it may, for a time, require a few pounds of water, but oxidation soon fills up any such small opening, sealing it perfectly. It would seem incredible, but it is a fact, that in this apparatus a stream of hot water which is not thicker than your little finger, carries the heat 700 feet, and then returns to the boiler at the temperature of 100 degrees. The boiler is a coil of tube of the same diameter with the conducting pipe. From the extent of that magnificent building, the British Museum, the expenditure this year, on the heating apparatus, will be upwards of $6000.

Prior to the ascertaining the proper strength for the pipes, in the early trials, there were a number of explosions, but not such as produced any injury beyond that to the pipe.

I have several other things on the tapis, which it is my intention to communicate to you at an early day. For some of them I am preparing to obtain patents, which will soon be matured. Yours, &c.

J. Perkins.

Physical Science.

Igniting Gunpowder by Electricity.

An easy method of Igniting Gunpowder by common Electricity, by Chas. P. Page, M. D. of Salem, Massachusetts.

Having often witnessed the futile attempts of experimenters to ignite gunpowder by small Leyden jars, I presume the following communication may be of value. I have been in possession of the fact for six years, but have never been able to fix upon a rationale. In common experiments for igniting gunpowder by electricity it is found necessary to introduce iron filings, or detonating powder, or to use very large jars, or batteries. But by a simple arrangement, powder may be exploded by a half pint or smaller jar; and what appears most singular in the experiment, there is no audible discharge of the jar; and when examined after the experiment, it is found still highly charged. An effectual method which has long been in use is that of passing the charge through water, but in this case also large jars are requisite. If a piece of wetted cotton thread be introduced in any part of the circuit, the powder may be ignited by a few inches of coated surface. The experiment is best regulated by Lane's discharging electrometer, and the length of the thread should be in proportion to the distance between the ball of the electrometer and that of the jar; that is, the thread should be so long that the jar cannot be discharged over its surface. From the curious result of this apparently retarded discharge, it was at first thought that it might have assumed somewhat the character of a galvanic current. But though frequently examined by the galvanometer and fine platinum wires, I have not detected any analogy.

We give place with pleasure to the above communication, although the experiment of restricting the electric current to a moistened thread for the purpose of more easily firing gunpowder is not new to us. It has been practised also by several experimenters of our acquaintance; among whom are Dr. Hare and Professors Johnson and Green, of Philadelphia, and doubtless by many others conversant with the progress of experimental science. We delayed inserting the paper until we could refer the author to the places where he may obtain more precise information on the interesting facts to which he refers.

In the 67th vol. of the Philosophical Magazine is a paper by W. Sturgeon, describing the difficulties he had met with, and claiming the discovery of the moistened thread. He mentions several variations of the experiment. Two years before this, Charles Woodward in a short paper in the 7th vol. (new series) of Thompson's Annals, showed an improvement on Cavallo's method of firing gunpowder by using small tubes containing water, as part of the circuit, and ascertained the instructive fact that it was immaterial whether the water is on the positive or negative side of the powder to be inflamed. In the 68th volume of the Philosophical Magazine, Thomas Howldy proves that powder may be inflamed by using a wooden point in the discharge, and that the gunpowder will explode even when spread on a cake of ice. Sturgeon, in the first volume of the Philosophical Magazine and Annals, comments upon the papers of Woodward and Howldy, and in the same volume is a rejoinder by Howldy. The statements of these gentlemen lead to the conclusion that when an electric discharge fails to ignite gunpowder, it is, in general, because the mechanical force of the current acting on the air disperses the grains before they are ignited, and that by retarding the velocity of the current, in causing it to pass through a filament of water or other imperfect conductor, the heating effect of the current becomes much greater in the immediate line of its track, and thus by its confined and sustained action becomes sufficient, though in very small *quantity*, to inflame the sulphur and ignite the powder. It may be that this is the sole explanation which the facts require. We have yet however much to learn in relation to the nature of electrical agencies. Dr. Faraday has shown, in the third volume, page 353, of the London and Edinburgh Philosophical Magazine, that in the cases alluded to, (the passage of common electricity through a moistened thread) there is a galvanic action fully sufficient to affect the needle of a galvanometer. The young electrician will find the several papers we have here cited worthy of his attention. G.

Progress of Practical and Theoretical Mechanics and Chemistry.

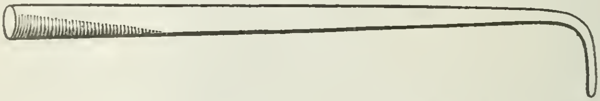
Suggestion for the use of the Blowpipe by working Miners. By JOHN PAIDEAUX, ESQ.

It is a leading principle of the present day, that artisans of every kind should be instructed, by means of Mechanics' Institutes, or other mediums of communication, in the scientific, as well as practical, elements of their respective arts: a principle, the benefits of which not being confined to its particular objects, must necessarily, in thus multiplying operative intelligence, advance the arts and manufactures of the country.

In conformity with this principle, I am induced to offer some suggestions for the easy and effectual application of the blowpipe to metallurgy, by working miners; a class of men who have hitherto received but little assistance from the scientific world. If the practice should gain ground amongst them, the satisfaction of the individual workman, in understanding the quality of every ore he encounters, and in having it in his own power to examine any mineral he may suspect to be metalliferous, is but one part of the advantage that may be anticipated. When it is remembered that the grey sulphuret of copper, the richest ore of that metal, was, until of late years, thrown on the mining heaps, for mundic; and that even all the copper ores were rejected for centuries, by the tinners, as worthless, how many other substances may we not hope to save from the waste heaps, or the deads below, when the thousands of persons engaged in working the mines shall have their curiosity and cupidity thus excited?

The simple instruments and materials hereafter described, will be easily procured and generally sufficient. The miner who desires further instruction may have recourse to a little book, containing, in small compass, and at small cost, much valuable information, "Griffin on the Blowpipe;" or the more scientific work of Berzelius, translated by Children, which, however, requires more chemical knowledge.

For ordinary metallurgic assays, the common blowpipe does very well: a mere tapering tube ten inches long, half an inch diameter at one end, and the opening at the other scarcely equal to admit a pin of the smallest kind, the smaller end curved off, for 1½ inch, to a right angle. A bulb, at the bend, to contain the vapour condensed from the breath, is useful in long operations, but may generally be dispensed with. In selecting the blowpipe, the small aperture should be chosen perfectly round and smooth, otherwise, it will not command a good flame.



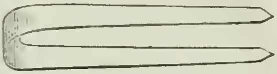
A common candle, such as the miner employs under ground, answers very well for the flame.

To support the subject of assay, or "the assay," as it has been happily denominated by Children, two different materials are requisite, according as we wish to calcine or to reduce it. For the latter purpose, nothing is so good as charcoal; but that from oak is less eligible, both from its inferior combustibility, and from its containing iron, than that from alder, willow, or other light woods.

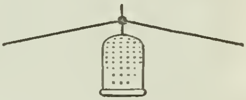
For calcination, a very convenient support, where platinum wire is difficult to procure, is white baked pipe-clay, or china clay, selecting such as will not fuse, nor become coloured, by roasting with borax.

These supports are conveniently formed by the process of Mr. Tennant. The clay is to be beaten to a smooth stiff body; then a thin cake of it, being placed between a fold of writing paper, is to be beaten out with a mallet, to the thickness of a wafer; and cut, paper and all, into squares of $\frac{3}{8}$ inch diameter, or triangles about the same size. These are to be put into the bowl of a tobacco-pipe, and heated gently till dry; then baked till the paper is burnt away, and the clay left perfectly white. They should be baked in a clear fire, to keep out coal-dust and smoke as much as possible, as either of these, adhering to the clay plates, would colour the borax in roasting. A small fragment of the bowl of a new tobacco-pipe will serve instead, in the absence of a more convenient material.

A simple pair of forceps, to move and take up the hot assay, may be made of a slip of stiff tinplate, 8 inches long, ½ inch wide in the middle, and one-sixteenth of an inch at the ends.



The tin being rubbed off the points on a rough whet stone, the slip is to be bent until the ends approach each other within ½ an inch, and the two sides are parallel; thus there will be spring enough in the forceps to open and let go the assay, when not compressed upon by the finger and thumb.



A magnetic needle, very desirable to ascertain the presence of iron, is easily made, of the requisite delicacy, where a magnet is accessible. A bit of thin steel-wire, or a long fine stocking needle, having ¼ inch cut off at the point, is to be heated in the middle, that it may be slightly bent there; thus:

While hot, a bit of sealing-wax is to be attached to the centre; and the point which was cut off, being heated at the thick end, is to be fixed in the sealing-wax, so that the sharp end may serve as a pivot, descending about ⅛ inch below the centre: taking care that the ends of the needle fall enough below the pivot to keep it from overturning. It must now be magnetised, by sliding one end of a magnet, half-a-dozen or more times, from the centre to one end of the needle; and the other end, a similar number of times, from the centre of the needle to its other end. A small brass thimble (not capped with iron) will do for the support; the point of the pivot being placed in one of the indentations, near the centre of the top; when, if well balanced, it will turn until it settles North and South. If one side preponderate, it must be nipped until the balance be restored.

A black gun-flint is also occasionally used to rub the metallic globules (first attached, whilst warm, to a bit of sealing-wax,) and ascertaining the colour of the streak they give. Thus minute particles of gold, copper, silver, &c., are readily discriminated.

A little refined borax and carbonate of soda, both in powder, will complete the requisites.

Having collected these materials, the next object for the operator is, to acquire the faculty of keeping up an unintermitted blast through the pipe, whilst breathing freely through the nose.

This is easier to learn from example, than from written instructions; yet, with attention, I believe the following will be found generally sufficient.

Any one free from nasal obstruction, can breath freely through the nose, with the mouth closed. Having done this for a dozen successive respirations, let him inflate the cheeks, and keep them so by pressing the tongue against the roof of the mouth. In this manner he will immediately, or very soon, be able to breathe through the nose just as freely with the cheeks inflated, as in the ordinary state.

When he can do this, without effort, the large end of the blowpipe is to be taken between the lips, and the mouth inflated as before. The pressure of the cheeks will thus force a stream of air through the pipe; and the mouth will quickly become nearly emptied.

It must then be inflated a second time, by forcing in air from the lungs, over the tongue, and again suffered to empty itself through the pipe, by the pressure of the cheeks; continuing the respiration all the while, at ease, through the nose. When this has been repeated several times, so as to have attained the power of breathing calmly, whilst the current through the blowpipe is produced entirely by the action of the cheeks; the next step is made by forcing in air over the tongue, *before* the mouth has completely thrown out the air it contains, and whilst the current is still continuing through the pipe. Thus he may keep up a continuous blast for a quarter of an hour, or more, without impeding his respiration, and with no other fatigue than of the lips where they press the pipe.

The next lesson is the regulation of the blast. For this purpose a candle with a longish snuff is to be lighted, and placed in a sloping position; the upper part of the wick being bent down nearly horizontal toward the same side as the candle leans. It is to be so placed that the operator, with both elbows leaning steadily on the table, can apply the point of the blowpipe just behind the flame, so as to blow it along the bent wick.

The blowpipe should not quite touch the flame, and should direct it a little clear of the wick, that the jet may be round and smooth. When properly produced, it will consist of a clear blue-pointed flame inside, with a yellow, or reddish, transparent outer one. If the outer flame is bright, dense, and sooty, like the ordinary flame of a candle, it indicates that the blast must be stronger, or that the aperture of the blowpipe must be enlarged, or brought closer to the flame; unless a smaller candle be employed.

Now, supposing the operator to have produced a clear, smooth flame, which does not smoke the edge of a sixpence placed at the blue point within it, let him rest the bend of the blowpipe, as well as his elbow, so that the instrument may remain perfectly steady.

He must now practise until, by regulating the pressure of his cheeks, and the filling of his mouth from the lungs, he can keep the jet, without puffs and jerks, as constant and firm as the pipe itself.

Let him next make a little cavity (two or three times the size of a split pea) in a piece of charcoal, and putting therein a fragment of malachite (green copper) not much larger than a mustard-seed, bring it up to the flame, so that the inner blue point shall act directly upon it: in a few seconds he will find it reduced to a bead of malleable metallic copper.

Continuing the blast, let him remove it into the outer flame, almost to the extreme point: he will soon find it covered with a crust; and, if put on one of the pipe-clay squares, it will be calcined throughout by the action of the external flame, and crumble under the blow of the hammer. Let him treat in the same manner a fragment of carbonate of lead: he will find it reduced still more readily in the blue flame, and calcined with almost as much ease in the external one.

Hence he will learn the third lesson, that the flame answers the double purpose of calcining and reducing furnaces; the outer flame being the calciner, the inner one the reducer. And this is the most important point of blowpipe knowledge.

The reducing action is increased by removing the blowpipe a little farther from the wick, so as to give the external flame more density, and a slight tendency to smoke the assay. The calcining action, on the contrary, is promoted by putting the point of the pipe quite into the flame of the candle, so as to make the outer flame of the jet very transparent, and only just visible. A little harder blowing is suitable for calcination than for reduction.

Most of the ores having a metallic aspect (as yellow and grey copper, lead ore, mundic, white cobalt, &c.,) contain sulphur, and require much calcination before being reduced: otherwise, the lead may be deficient in quality or quantity, or in both. Some of them fly on being heated, in which case the flame must be brought upon them gradually, letting it first pass over them, and raising them very slowly into it.

But there are some which decrepitate and fly about, when heated even in this cautious manner. With these it is necessary to use a second piece of charcoal as a cover. For this purpose, taking two pieces, each having a flat side, two inches long, let a groove be cut along one of them about a quarter of an inch wide and deep, and a cavity of the usual size, in the middle of the other. Place in the cavity a fragment of assay, four or five times as large as that usually employed, and put the grooved piece over it, so that the flat sides shall coalesce, and the cavity fall under the groove. The blowpipe jet, being then sent along the groove, will soon cause the assay to decrepitate, whilst the greater part of the projected fragments will be caught in the groove.

As soon as the decrepitation has ceased, the jet should be discontinued, and the assay thrown out on a plate. A particle will then most likely be found, large enough to work upon; but if not, the operation may be repeated on a larger piece, or a good knob of it heated red, in the bowl of a tobacco-pipe, covered, in a common fire. After once being heated red, the assay is no more subject to decrepitation.

In calcining ores, care should be taken not to melt them, as that retards the process: the heat should be applied at first gently, keeping the assay beyond the reach of the flame, and approaching as the sulphur is driven off, and the assay becomes less fusible. This process should be performed on the pipe-clay cupells.

When the sulphur, &c., is entirely driven off (which is sometimes a good while after the smell has ceased,) the assay should be removed to the charcoal, and tried in the reducing flame; a little soda being added if reduction does not readily ensue. If it still refuse to yield a metallic globule, it may be cooled quickly by being dropped on a knife-blade, or into a silver spoon, and then taken up on a bit of wax, and tried with the magnetic needle. If, on the approaching, or touching, one of the ends of this instrument when mounted as above described, a strong attraction be manifest, the assay is probably iron, which may be farther proved as hereafter stated.

If it do not produce a bead, or if the bead produced be not easy to recognise by itself, or by the streak it produced on a black gun-flint, a minute portion of it is to be melted with a little borax, in the reducing flame, on a clean surface of the charcoal; taking the precaution to melt the borax first, and see that it produces a colourless, transparent bead. If it be either coloured or opaque, after good fusion, it is unfit for the purpose; and another must be made, at a different part of the charcoal. The borax will swell and twist (like burning leather) in melting, but is easily brought back to a bead, by a little management of the hand.

If, when a portion of the assay is melted in it, the borax remain colourless and transparent, more assay is to be added, and the addition continued until it manifest some colour, or can no longer be rendered transparent; or, if the colour be too dense to be distinguished, more borax must be used.

A portion of borax is then to be fused upon a pipe-clay cupell, over the surface of which it will spread like a glaze, and will impair its whiteness, but must exhibit no particular tint of colour: if it do, another piece must be employed.

A minute portion of the assay, or, rather, of the borax bead containing it on the charcoal, is now to be added, and well fused on it, in the reducing flame, increasing the dose of assay or of borax, as before, unless colour be manifested.

Some of the metals are volatile, and will give neither bead nor colour to borax, but evaporate before the blowpipe in a white smoke, often of a strong odour; sometimes covering the charcoal with a white powder, or depositing a circular halo upon it, white or coloured, at a little distance round the assay. This halo may be again driven farther, or dispelled, by the action of the flame; generally of the reducing flame, but sometimes of the other. By these characteristics the volatile metals may be distinguished, as effectually as if they left a bead.

It happens also, frequently, that a volatile metal is combined with one that is fixed; as in white mundic, composed of arsenic and iron. The blowpipe then separates them; the arsenic, driven off, is known by its smell, and the iron, remaining, by its action on the magnetic needle.

But when two or more volatile metals are united, or two or more of the fixed kind, they are less easy to discriminate, and require both practice and study.

The following rules may generally be relied on; but in cases where the assay gives ambiguous or complicated indications, proportionate caution is necessary both in operating and deciding.

The miner will commonly judge a mineral to be metallic, if it has a metallic aspect, not destroyed by scraping; or if it feels particularly heavy in the hand. But others also discover metallic properties to the blow-pipe, by

1. Yielding a bed of metal, to the reducing flame, on charcoal; which is facilitated by the addition of soda.
2. Passing off in vapour, more or less dense.
3. Attracting the magnetic needle, after heating in the reducing flame, on charcoal.
4. Colouring borax strongly, either on charcoal or pipe-clay.

(TO BE CONTINUED.)

The Preparation of China Clay.

"An intelligent foreigner says, in traveling from Paris to St. Petersburg, from Amsterdam to the furthest part of Sweden, and from Dunkirk to the extremity of the south of France, one is served at every Inn, upon English (earthen) ware. Spain, Portugal, and Italy, are supplied with it; and vessels are loaded with it for the East Indies, the West Indies, and the continent of America." * * * "The manufacturing part alone in the potteries, and their immediate vicinity, gives bread to 15 or 20,000 people, yet this is but a small object, when compared with the many others which depend on it."—*Lardner's Cyclopædia*, xxvi., 16-20.

Mr. Barlow in the "Encyclopædia Metropolitana" states that the Cornish china-clay forms from eight to twenty-eight per cent., of *body* of porcelain, and the china-stone from four to eight per cent., whilst in the *glaze* from twenty-three to forty per cent. of the stone is employed.

Dr. Boase says that the quantity of china-clay exported from the neighbourhood of St. Austel "exceeds 7000 tons per annum: and when to this is added the amount of china-stone, which is about 5000 tons more, the whole forms a very important branch of commerce. In estimating the value of these works to the community, it must also be remembered that they are situated in bleak barren moors; thus affording employment and support to several hundred persons, where the impracticable soil would hardly maintain a single family."—*Cornwall Geol. Trans.* iv. 236-7.

There are three districts in this county in which the china clay and china-stone, occur, viz., in the parishes of St. Austel, St. Mewan, St. Stephens, St. Dennis, and Roche, all near St. Austel: in the parishes of Breage, and Germoe, near Helston: and in the parish of Towednack, near Penzance. The two latter are, however, quite insignificant, when compared with the first named.

"The china-stone is a kind of granite, the felspar of which has undergone a partial decomposition: it is carefully selected, so as to be entirely free from schorl, and requires no other preparation for the market than to be broken into a size convenient for carriage. This granite is of a peculiar nature; it does not contain any mica, but numerous glossy scales of greenish yellow talc. * * * These beds of porcelainous granite are very numerous. I could not ascertain their bearings in every case: they were, however, very commonly from north to south, or rather east of north, and west of south." —*Boase. Cornwall Geol. Trans.* iv. 236.

In all granite districts, observes Dr. Turner, "Phil. Mag." (1833, iii. 22.) the formation of porcelain clay from the rocks, rich in felspar, exemplifies in a striking manner the affinities of water and carbonic acid, for potash and soda, and that of oxygen, for iron.

"It is probable that the long continued action of pure water might produce decomposition; but the effect of its affinity for the alkalies of the rock is materially aided by that of carbonic acid for the same basis. This is shown by the increased decomposing power of water, when charged with carbonic acid, and by the action of moist carbonic acid gas on granite."

It has been seen by the foregoing extracts, that the prevailing opinion is that china clay is derived from the decomposition of the felspar in a particular kind of granite; our object will now be to describe the operations pursued for the separation of the clay from the other constituent minerals of the rock, with which it is found mixed.

The vegetable mould and rubbish (meat-earth, and overburden) being removed, several pits, or tanks, are prepared and rendered water-tight: they are so placed that a communication may at pleasure be made, or stopped, between almost every one of them.

The first two or three of these are about ten or twelve feet in length, and three or four in breadth and depth; the others are much larger, say from thirty to forty feet long,—fifteen to twenty feet wide, but of much the same depth as the first.

A stream of water is now introduced at the highest part of the spot intended to be worked, and a fall of a few feet is preferred, in order to facilitate its action on the friable material; which is not unlike mortar in appearance, and contains abundance of siliceous gravel; much as quartz is disseminated in the parent granite. The action of the water on the clay is assisted by the workmen, who agitate it with shovels, pick-axes, and other tools; and it is thus carried, in suspension, into a furrow, through which it is conveyed into the pits or tanks. Much of the silicious and other ponderous matter subsides in the channel, and in the first pit; whilst substances of smaller gravity are carried into the adjoining ones (the *mica-pits*,) and the fine clay, being more completely suspended in the water, is carried into the larger receptacle, where it gradually subsides, and the colourless liquid is permitted to escape through apertures prepared for the purpose, at small heights above each other, so as to allow of its draining off, without disturbing the sediment of clay.

In tempestuous weather, however, the subsidence of the clay occupies a very much longer time than ordinary, and artificial means are employed for effecting it. A solution of alum is sprinkled on the surface of the turbid liquid, from a common garden watering-pot, which occasions the clay to fall down very rapidly.

When a sufficiently thick deposite of clay has been thus obtained in the tank, as much as possible of the supernatant liquid is allowed to run off, and the clay is removed to another place, where being more thinly spread, evaporation is facilitated. This is accomplished by means of a common lifting pump, of small size, which is introduced into the first receptacle, and empties itself into the second. The mud, however, is so thick, that it will not readily flow towards the pump, which is scraped by a toothless wooden rake; and *even after it has been raised by the pump, it is so dense as to require being raked therefrom, and into the second pit.*

When the moisture has evaporated sufficiently to bring it to a proper state of consistence, it is cut into rectangular lumps, of from six to twelve inches square, which are (as the season may be,) removed either to the adjacent plain or into the drying-house, which is a shed open at the sides, and contains frames on which the clay is so placed as to permit a free circulation of air. When dry, the clay is scraped free from all impurities, and packed in cases for the market.

The best qualities are perfectly white, but the inferior have some slight tinges of yellow; all are, however, free from the smallest traces of grit.*

It is exceedingly curious, that those who are conversant with the preparation of china-clay, will readily ascertain the position which each end of a given lump occupied with respect to that of the pump, by which the mud was drawn from one tank into another. Because, when scraped with any sharp instrument in a direction contrary to that which it flowed, a smooth surface cannot be obtained; whilst, when the direction of the current and of the scraping are coincident, it readily presents an even surface.

There is something very interesting in this fact, which, if properly understood, might perhaps throw some light on the bedding and lamination of rocks, and other intricate problems of geology.

Much has been said on the *causes* of the decomposition of the felspar in the granites (protogenes,) which contain talc instead of mica, but there seems good reason for the opinion of Dr. Boase ("Phil. Magazine" for the present month,) that it is owing to some peculiarity in the composition of the felspar itself. This excellent analyst has examined some of the china-clays of Cornwall, and finds

	St. Stephens.	Breage.
Silica, . .	39.55	40.15
Alumina, ...	38.05	36.20
Magnesia, ...	1.45	1.75
Water, ...	12.50	11.66
Insoluble residue, quartz and talc, .	8.70	9.50
	100.25	99.25

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* The drying pits are covered with the refuse quartzose sand, previously to being used, in order to prevent the clay from adhering to the bottom.

The inquirer may find much instructive matter in all the authorities we have quoted; and as well in Berzelius, *Traite de Chimie*; Becquerel, *Traité de l'Electricité*; and Berthier, *Annales de Chimie*, lxii. 225. Mining Journal.

Memoir on an apparatus for evaporating and concentrating Saccharine Juices at a low temperature. By M. Degrand, Civil Engineer at Marseilles.

The author of this memoir obtained the prize of 4000 francs offered by the "Society of Encouragement," for the construction of an apparatus that would evaporate saccharine solutions under a pressure of two-tenths of an atmosphere. The memoir is contained in the Bulletin of the Society, for July 1836. Two large plates, descriptive of the apparatus, accompany it, but they are executed in too elaborate a manner for the purpose of this Journal. We shall therefore endeavour, by a verbal description, as perspicuous as we can render it, to furnish our readers with an intelligible account of this ingenious scheme for performing evaporation under reduced pressure.

The boiler is of a globular form (though not necessarily confined to this figure) consisting of two hemispheres, fastened together when in action by flanches and screws in the usual way. The lower hemisphere is lined with the coils of a worm for the admission of steam from a generator, the heat of which carries on the evaporation. This worm (or there may be more if needful) is of course immersed in the fluid to be evaporated. The hemisphere has likewise a double, or false, bottom, leaving a space into which the steam also enters.

When the boiler is ready to receive the juice, it is to be exhausted of air either by the action of an air pump, or in any other convenient way. One of the methods described in the memoir, and which appears to be the most eligible, is to fill the boiler with steam from the generator, allowing the air to escape through a cock, all the other passages being closed. The stopper of a tube proceeding from the upper half of the boiler, and terminating in a cistern containing the juice to be evaporated, is then opened, and as the boiler cools, the pressure of the atmosphere forces the juice into the boiler until a float within it, indicates the requisite quantity. The boiler contains two sight holes, or windows, formed of thick glass, through which the internal operations can be observed. When the ebullition becomes too rapid, a small quantity of oil is admitted from a cup placed over the boiler. Steam of the temperature of about 112° Cent. = 234° Fahr. being then admitted into the worm and the false bottom, ebullition soon commences and the evaporation rapidly proceeds.

The vapour from the juice passes into a tube which proceeds vertically from the top of the boiler to some distance, and then turns at right angles and enters horizontally into the upper opening of a long worm, which is placed erect within a vertical cylinder which encloses it on the sides, but which is open at top and bottom, serving, of course, as a kind of chimney through which, while the worm is hot, there is a continually ascending current of air.

The horizontal position of the tube which conducts the vapour from the boiler to the refrigerating worm, is surrounded by a metallic sleeve, or muff, for receiving the water, which by falling on the outside of the worm in a continual shower is to condense the internal vapour. This sleeve is of course joined to the tube in an air-tight manner. The water is admitted into the cavity of the sleeve through a tube from a reservoir placed above it. This water soon acquires the temperature of the horizontal tube itself. It passes from the sleeve through a separate tube, and is discharged into a circular sieve situated immediately over the refrigerating worm, and falls in a fine shower upon the worm, trickling down upon it to the bottom. The ascending current of air through the open cylinder which surrounds the worm, carries off the external vapour and promotes the refrigeration. The rapidity of this current may be accelerated by increasing the height of the cylinder, or by connecting its top with a neighbouring chimney, or by a ventilating drum, or by a fire at the bottom. To produce a maximum of effect in the refrigerator, the ascending current of air must be at a maximum, and as this velocity is dependent on the excess of temperature of the aqueous shower, and the coils of the worm, over that of the external atmosphere, it follows that the temperature of the descending water ought, at its source, to be at a maximum. Now, this can be the case only when the source of the shower is at the same temperature as the vapour to be condensed. If less hot, it will draw in less air, if hotter it would raise the temperature of the vapour to be condensed. Consequently (says the inventor) the water employed in the cylinder, as the agent of refrigeration, produces a maximum of effect when it is as warm as the vapours it is destined to condense.

If (he adds) this simple proposition be announced,—"that in refrigerating with water, the less of it will serve when it is warm than when it is cold; and that there is the least expenditure of water when it is as hot as the vapour on which it is to operate,—this proposition would appear very singular; and yet, it is conformable to the laws of nature and appears strange only, because, until the present time, cold water only has been employed in refrigeration."

It is for this reason that the warm water which is pumped into the reservoir is passed into the sleeve, for the purpose of being heated as nearly as possible to the temperature of the vapour, before it is poured into the sieve and thinly distributed over the worm. That which escapes evaporation, in trickling over the worm, is collected at the bottom of the cylinder, and is pumped back, while still tepid, into the reservoir.

It would thus appear that the evaporation of the hot water which is distributed over the worm, aided by the current of air through the cylinder, carries off more heat, and condenses the internal vapour more rapidly, than would be effected by cold water; and that this evaporation and condensation are so effectual as to sustain the ebullition of the sirup under the reduced pressure required by the conditions of the premium. The apparatus being exhausted of air in the commencement, and no ærated water being allowed to enter, it is sufficient (says the memoir) in order to evaporate the saccharine solution under a tension of vapour equal or inferior to 0.2 of atmospheric pressure, that the refrigerating surface of the worm and the mass of air which is drawn into the cylinder, be in harmony with this effect.

When the sirup is sufficiently reduced, the boiler is evacuated without allowing the air to enter. This is effected by expelling the air from the vessel into which the reduced sirup is to be emptied ; when on opening the communication the sirup runs into this vessel, the passages are cleared of air, a fresh quantity of juice is admitted into the boiler, as before, and the process is continued, and thus a greater number of runnings can be performed in a given time.

The mode of sugar boiling above described is considered by the inventor to be a great improvement on the steam process of Howard. It may be characterized, in contradistinction to all other kinds of apparatus for the evaporation of saccharine solutions, by the brief remark,—*that it refrigerates with hot water, and expends only a minimum of water.*

This new process has received the sanction of experience, and is successfully conducted on a large scale in various refineries.

Instead of effecting the condensation with water, the idea occured to *Charles Derosne*, to employ the clarified juice itself. This is distributed over the cylinder by means of the sieve, as before described; and, without increasing the fuel employed, it economises the process to the amount of 40 per cent. G.

On some Silicates of Alumina. By R. D. Thomson, M. D.

To the compounds of silica and alumina great interest is attached, in consequence of the frequency of their occurrence, the diversity of their external characters, even when closely allied by chemical composition, and their utility in manufactures.

Notwithstanding the care which Berthier has bestowed on the subject, the arrangement of this class of minerals labours under great disadvantages, and it is questionable whether our present knowledge be adequate to enable us to introduce that precision which has been applied to other bodies possessing analogy of chemical composition.

I was led to the investigation of the composition of these simple compounds, from having lately met with three species of minerals, which appeared to possess distinctive external characters, but could not be referred by these features, with certainty, to any described species. They were all found in rocks connected with a red sand-stone, which is deposited in horizontal strata along the banks of the Tweed, in the neighbourhood of Melrose, and appears to be referable to the old, red, sand-stone series, or transition formation, being occasionally interrupted by dykes of green-stone and clay-stone porphyry. In the latter of these trap rocks, two of the minerals alluded to are found in considerable quantity, while the third, seems to occupy a place in the sand-stone itself.

I shall describe their characters and composition, and compare them with the simple compounds of silica and alumina which have been examined. The first of these minerals, I have termed *Tuésite*,* from Tuésis the river Tweed. It occurs in veins in porphyry, or indurated sand-stone slate, which is intimately connected with the old, red, sand-stone. Its colour is milk white; opaque; lustre, dull; sectile. Hardness 2.5. Specific gravity from 2.434 to 2.558.

Before the blow-pipe *per se*, it becomes blue and brittle, fusing with carbonate of soda into an opaque bead, and with borax and salt of phosphorus into a transparent glass. It forms an excellent slate pencil. A portion of the mineral was finely pulverized and fused with carbonate of soda. The silica being separated in the usual manner, the alumina was precipitated by caustic ammonia in the form of beautiful white flocks, which after determining its weight, was dissolved in sulphuric acid, with the addition of potash. Regular crystals of potash, sulphate of alumina, were the result of the gradual evaporation of the solution. The liquid remaining after separating the alumina was precipitated by oxalate of ammonia. The product was a small quantity of lime. The residual liquor was evaporated to dryness, the dry salts heated to redness, dissolved in pure water and boiled with carbonate of soda; a precipitate of magnesia ensued. This precipitate being weighed, was dissolved in dilute sulphuric acid. The whole of it dissolved, with the exception of a minute portion of silica, scarcely appreciable, and which produced an amber coloured bead when fused by the blowpipe with carbonate of soda.* The water in one trial amounted to 13.5 per cent., in another to 13.2.

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* See description, Thomson's Mineralogy, vol. i. 214.

The constituents are

Silica,	44.300	4 atoms
Alumina,	40.400	3 „
Lime,	0.755	
Magnesia,	0.500	
Water,	13.500	2 „
	99.455	

The formula, to represent its composition, which I am disposed to adopt, is

$$2\ \mathrm{Al}\ \bar{\mathrm{S}} + \mathrm{Al}\ \mathrm{S}^2 + 2\ \mathrm{Aq}.$$

Accordin to Berthier's view it would be,

$$2\ \mathrm{Al}\ \mathrm{S}^2 + \mathrm{Al}\ \mathrm{Aq}.$$

If, however, the lime and magnesia were taken into account, and supposing them to re-place alumina, we should have, according to the formula of Professor Thomson,

$$3\ \mathrm{Al}\ \mathrm{S} + 2\ \mathrm{Al}\ \mathrm{S}^2 + 3\ \mathrm{Aq}.$$

Corresponding with this composition, we find several analyses; especially one by Boussingault, of a Halloysite, as he terms it, from Guatequa in New Granada, found in carbonaceous schist with anthracite, of a soft consistence, with a cheesy fracture, becoming transparent in water; and two of Kaolin by M. Berthier.

	Halloysite from Guatequa†	Kaolin. St. Yriex.‡	Schneeberg.§	
Silica,	45	46.8	43.6	= 4 atoms.
Alumina,	40.2	37.3	37.7	3 "
Potash,		2.5		
Peroxide of iron,			1.5	
Water,	14.8	13.	12.6	2 "
	100.0	99.6	95.4	

It is quite obvious that these correspond with the formula which we have already given. The halloysites of Berthier differ essentially in their external character from tuesite. The specimen from Anglar, near Leige, possesses a density of 1.8 to 2.00, or, according to Ingelspach Lariviere, from 1.82 to 2.09.‖ Fracture compact, waxy, conchoidal, pure white, blueish, scratched by the nail, taking a polish under the finger. It is found in veins of hydrate of iron mixed with galena, carbonate of lead and calamine, which traverse transition limestones, in masses varying from the size of the fist to that of a cubic metre. Two specimens afforded,*

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* This is a peculiar state of silica well known to chemists as occurring in the latter stage of analyses, which has frequently been mistaken for Titantic acid.

† Ann. de Chim., liii. 439.

‡ Traité des Essais par la Voie Seche par M. Berthier, i. 58. § Ib.

‖ Ann. des Mines, v. 310.

	Anglar.	Hall.	
Silica, .	39	39	4 atoms.
Alumina, . .	34	35	3 "
Water, .	26	25.5	5 "
	99	99.5	

Their formula is,

$$2\,Al\,S + Al\,S^2 + 5\,Aq.$$

If then the lime and magnesia be considered foreign to the composition of tuesite, we shall have halloysite forming with it a sub-species of the same mineral, but possessing decidedly distinct characters, apparently occasioned by the addition of 3 atoms of water. And it seems necessary that the term halloysite should be restricted to those compounds consisting of silica and alumina, in the proportion of 4 atoms to 3, with 5 atoms of water, while tuesite contains the same proportions of solids but possesses only 2 atoms of water.

2. *Bisilicate of Alumina or Fuller's Earth.*—This mineral is found in round masses in the bed of a stream, associated with clay-stone porphyry, near Maxton. Sp. Grav. 2.394.

With nitre, soda and salt of phosphorus, fuses before the blowpipe into an opaque mass. With borax, fuses into a transparent bead, pale amber coloured when hot, colourless when cold. Colour yellowish white; fracture earthy, soft, soiling the fingers; scratched by the nail, tuésite, and sulphate of lime; adheres to the tongue like halloysite; contains crystals of decomposing felspar interspersed through the mass. Its constituents I ascertained to be,

Silica, . .	57.105	4 atoms.
Alumina, .	31.850	2 "
Magnesia, .	2.615	
Water, .	7.280	1 "
	98.850	

and its formula $2\,Al\,S^2 + Aq.$

Berthier has included a mineral possessing exactly the same composition, with less water, under halloysite, and another under kaolin, as is exhibited in the following table:†

	Halloysite. Fahlun.	Kaolin. Normandy.	
Silica, .	46.8	50	2 atoms.
Alumina, .	26.7	28	1 "
Peroxide of iron,	5.0	5.5	
Magnesia, .	0.4	.7	
Lime, .	3.0	5.5	
Water, .	13.5	9.5	1 "
Potash, .	.	2.2	
	95.4	101.4	

Berthier considers these two specimens as affording instances of felspar in different states of decomposition, and although he states no circumstances which tend to establish his supposition, yet it is possible his conjecture may be correct. I could observe no fact, however, which could give the slightest countenance to the idea, that Tuésite or Fuller's earth, but more especially the former, is in any way connected with felspar.

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* Traite des Essais, i. 58.

† Traite des Essais, i. 60.

3. In the sand-stone, which has been already described, a soft, whitish substance occurs. It is smooth, yielding to the finger, containing greenish streaks, and answers to the description of lithomarge, or rock marrow. The specific gravity is 2.457. Its constituents are, according to my analysis,

Silica,	56.850	19.0 atoms.
Alumina,	25.000	7.5
Potash,	6.178	1.
Lime,	3.492	.5
Magnesia,	2.640	.5
Water,	5.840	4.
	99.900	

Its composition, including the lime and magnesia with the alumina, is expressed by the formula,

$$8\tfrac{1}{2}\,\mathrm{Al\,S^2} + \mathrm{K\,S^2} + 4\,\mathrm{Aq}.$$

Berthier analyzed two Kaolins which approach lithomarge in composition, magnesia taking the place of the potash.

Kaolin from St. Tropez, $3\,\mathrm{Al\,S^2} + \mathrm{M\,S^2} + 2\,\mathrm{Aq}$.

Kaolin from Mende, $4\,\mathrm{Al\,S^2} + \mathrm{M\,S^2}$.

For the sake of greater precision, I shall give a view of the composition of the various Halloysites and Kaolins analyzed by Berthier and Boussingault, including along with them the minerals analyzed by myself, and expressing their constitution by formula.

I. 1. Tuésite, $2\,\mathrm{Al\,S} + \mathrm{Al\,S^2} + 2\,\mathrm{Aq}$.

Under this species are comprehended Halloysite from Guatequa analyzed by Boussingalt, and two Kaolins from St. Yriex and Schneeberg, examined by Berthier, and Clay of Angleur.

2. Halloysite, $2\,\mathrm{Al\,S} + \mathrm{Al\,S^2} + 5\,\mathrm{Aq}$,
including the Halloysites of Anglar and Hall, and perhaps, Nontron.

II. Bi-silicate of Alumina or Fuller's earth,

$$2\,\mathrm{Al\,S^2} + \mathrm{Aq}.\ \text{and}\ \mathrm{Al\,S^2} + \mathrm{Aq},$$

expressing the composition of Halloysite from Fahlun, Kaolin from Normandy, and Fuller's earth from the Tweed. Kaolin from Meissen possesses less water $= 3\,\mathrm{Al\,S^2} + 2\,\mathrm{Aq}$.

III. Linzinite is the name which John originally gave to the Halloysite of Hall. It may, with propriety, be applied to the ter-silicates of alumina, whose composition is denoted by

$$\mathrm{Al\,S^3} + 3\tfrac{1}{4}\,\mathrm{Aq}.$$

Berthier analyzed two Halloysites from St. Sever and Confoleus possessing this constitution, besides clays from Hoeganas, Forges, Montereau, Cymolite, Cologne, with less water.

IV. Lithomarge, $8\tfrac{1}{2}\,\mathrm{Al\,S^2} + \mathrm{K\,S^2} + 4\,\mathrm{Aq}$.

Under this species, we may, perhaps, include the Kaolins from St. Tropez and Mende whose expressions have been already exhibited.

In presenting the preceding arrangement, it is only my intention to assist in simplifying the study of these interesting compounds; for it appears more beneficial to classify minerals, not according to theoretical views, but according to their actual nature. M. Berthier, whose analytical accuracy none will dispute, has thrown all the simple silicates of alumina into one class: but, it may be asked, are none of these compounds as much true species as other minerals with which we are acquainted? or, if they are different states of decomposing felspar, are they not in this respect analogous with many species, which have been presumed, with considerable probability, to be the products of decomposing rocks? Record General Science.

Sugar:—Examined in its Commercial and Chemical Relations. By George Guynne and James Young.

Before detailing the results of our experiments on sugar in its relations to chemical agents, it may be advisable to give a brief outline of "sugar refining," as it is now practised in Great Britain.

The various modes of sugar refining may be comprehended under two general divisions. First, where the raw material is clarified by blood and, perhaps, a little animal charcoal in fine powder, the water evaporated in a pan over the naked fire, and the remainder of the process of *whitening* performed by pouring over the *moulds* containing the sugar, a very thin paste of aluminous clay and water. This method is generally distinguished by the name of the *old plan.*

Second, where the raw material is in a great measure freed from impurities by being made into *meltings*, the meltings dissolved in water by the aid of steam instead of the naked fire; the evaporation performed in vacuo, and the remainder of the process of whitening performed by pouring over the moulds containing the sugar, a sirup, technically called *liquor*, made from refined sugar. This method is generally known by the name of the "patent process," or Howard's plan.

1. *Old Plan.*—Although the old plan is decidedly inferior to Howard's, in economy and speedy production of the refined material, it is still carried on to a considerable extent in London. The proprietors of such sugar houses are, with perhaps scarcely an exception, Germans, or the descendants of Germans, and much wedded to old habits. Although they have the experience of above twenty years before them of the successful working of Howard's plan, they still think their own method the better of the two, and expect to live to see the total failure of the other.

In the working of an "old plan" house, the raw sugar is mixed over night with lime and blood, in the proportion of about four gallons of the former to one of the latter for each hundred weight of sugar; next morning the fire is kindled under the copper, and by careful management, when coming to the boiling point, the insoluble impurities are entangled in the coagulative albumen of the blood and carefully skimmed off. This operation is called *clarifying*, and the mass of blood and impurities *scum*; when as much scum has been removed as possible, the sirup is filtered through a blanket, distended over a basket, to separate the flocculent matter which has escaped the *scumming ladle.* The "scum" is worked over again with water, in order to get as much matter out as possible. The sirup is now evaporated over the fire until it reaches the proof point. *Proof* is a technical phrase employed in the sugar-house, to denote that point in the evaporation of water from sugar, which will afford the greatest weight of crystals *(grain,)* consistent with a due draining of the sirups *(mother liquors,)* from the sugar, when put into moulds. Proof varies according to the quality of the sugar; where the sugar is of an inferior kind, it requires to be higher *(stiffer proof,)* than when of a better description. Although the thermometer is far more accurate, as an indicator of proof, than the *finger and thumb*, still few of the German boilers employ it; they look on it as new fangled toy, not to be depended on. Finger and thumb proof is performed by taking some of the boiling sirup from the copper with a small ladle, and putting it between the fore-finger and thumb, and then separating them, so as to form a thread; by the breaking of this thread the proof is taken. When a thermometer is employed, it is taken by single inspection of the scale, and it varies from about 232° Fah. to 248° or 250°, according to the quality of the materials; 232° would be *proof* of double refined loaves, and 248° the proof of the lowest quality of sugar "bastards;" when this takes place, the boiling fluid is laded into shallow copper vessels, called "coolers," where it is occasionally stirred, in order to promote the formation of the sugar-house crystals. After remaining the requisite time in the coolers, it is transferred into "moulds;" these are conical shaped vessels (usually unglazed earthenware, though iron is preferable,) open at top, with a small hole at the apex; the hole is stopped with a piece of paper, to prevent unconsolidated sugar from flowing out; next morning the paper plug is withdrawn, and the moulds placed in earthen ware, or iron, jars (pots.) In twenty-four hours about one-third of the weight of the contents of each mould will have passed into the pots in the state of *green sirups.* When the green sirups have properly drained, the upper surface of the sugar is scraped off to perhaps the depth of an inch (*brushing off*;) the "brushings off" are mixed with water into the consistence of paste and poured on the moulds. Some hours after this, a cream of aluminous clay and water is poured on the sugar; the water slowly parts from the clay, percolating through the sugar, a portion of which it dissolves, carrying with it the colouring matter and other impurities. This *claying* is repeated until the sugar is properly whitened. The sirups, which the claying forces out from the sugar, are distinguished by the names of *second sirups* and *drops.* The "green sirups" are evaporated separately and made an inferior sort of sugar *(bastards;)* the liquors which drain from the bastards constitute treacle, an article from which (although very sweet to the taste) no further crystals have as yet been produced. The "seconds sirups" and "drops" are used along with fresh material in making refined sugar.

The sugar, after draining completely on the pots, is turned with the face downwards for twenty-four hours, and then papered. It is now put into the drying stove (heated to about 135° or 140° Fah.) where, in a few days, the water it contained is entirely evaporated away, and it is now ready for sale.

The entire period which elapses from the time the sugar is "clarified" until taken from the stone averages, about forty days.

2. *Howard's Plan.*—The leading features of this plan are the substitution of steam, as a heating medium, instead of the naked fire, and the evaporation of the liquor performed in vacuo. Where the patent process is strictly pursued, the following are the different steps:—The "raw sugar" is put into a vessel with a "jacket" heated by steam (*melting pan.*) and some water added and well stirred; when the water reaches about 180°, it is transferred into large moulds where it is allowed to consolidate; the stoppers of the moulds are then withdrawn, and the green sirups separate from the sugar, leaving it comparatively pure. The sugar is now called *meltings*, which are put into a vessel heated by steam (*blow ups,*) and lime water, together with a little alomina (finings) added; after boiling for a few minutes the liquor is let into the filter to separate insoluble matters. When

the liquor becomes *bright* it is transferred into the vacuum pan, where the necessary degree of evaporation takes place. This operation is examined from time to time by means of a brass rod (*proof stick*,) with a small hole near its end; by turning the proof-stick, a cock is opened in the vacuum pan and some of the sugar brought away in the small hole. Crystals begin to appear at an early stage of the evaporation, and gradually increase in size until the full charge is in the pan. In the "old plan" no crystals show themselves until the "liquor" is laded into the "cooler," and the temperature has fallen to about 165°. When the requisite degree of concentration* has taken place, a valve is opened in the bottom of the pan, which allows the fluid sugar to escape into a copper vessel (*heater*,) heated by a steam jacket placed underneath. In the "heater" it is constantly stirred until the temperature rises to about 175°, when it is transferred into moulds, and next morning carried into the "curing floors," when the stoppers are withdrawn and the moulds placed in "pots," in order to allow the green sirups to drain.

The subsequent treatment is similar to that of the old plan, with the exception that instead of "claying," there is poured over the sugar a saturated sirup (*liquor*) made from refined sugar itself. This "liquor" whitens the sugar in the same manner as claying; and possesses the advantage of dissolving no part of the crystals, besides greatly economising the time. Instead of taking forty days to produce refined sugar, it is here produced ready for sale in about twenty days.

The green sirups produced from the meltings are made into bastard sugar; the green sirups from these constitute treacle. The green sirups which the refined sugar yields are made into an inferior sort of refined sugar (*brown lumps*,) and their green sirups make an inferior sort of bastard sugar. The "*second sirups*" and "*drops*" are worked in a similar manner to that described in the old plan.

Although more than twenty years have elapsed from the publication of Mr. Howard's plans, the only real improvement made since his time is a method of percolating the *raw* sugar liquor through an immense body of animal charcoal, in the state of coarse grains. This plan is now extensively employed in several refineries in this country, and supersedes the making of meltings, besides shortening the period of producing the refined sugar three or four days. The following is a brief description of the process as practised in one of the largest sugar-houses in London.

The "raw sugar" is dissolved in the "blow up" in the usual manner; a little finings and a very small portion of blood are employed, which materially assist the filtration; after boiling a few minutes, the liquor is filtered through *Schroder's bags*. From the *clear liquor* cistern the fluid passes into another apartment in which are several immense vessels, made of wood and lined with lead, nearly full of animal charcoal of the size of coarse gunpowder. The vessels have each a false bottom covered with cloth, and the charcoal is placed over the cloth to the depth of about thirty-four inches. The *clear*, but dark, coloured liquor is allowed to percolate slowly through the beds of charcoal. The first part is generally a little turbid from the presence of minute particles of charcoal; this turbidity soon ceases, and the liquor is then allowed to flow into the receiving cistern from whence it is transferred into the vacuum pans. If the charcoal be good, the liquor which flows through it is for some time as colourless as pure water, and perfectly free from every impurity possible to be removed.

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* When the air-pump and other parts of the machinery are in good order, the temperature of the "liquor" in the vacuum pan at the highest range is about 160°; and the vacuum produced equal to support a column of mercury 26 inches high.

It soon however becomes coloured, and in about seventy-two hours from commencing, no more liquor is allowed to flow on the "beds of coal." The liquor absorbed by the charcoal is obtained by repeated percolations of cold water.

The refinery above alluded to, works about 60 tons of sugar per week, and to carry on this work about 50 tons of charcoal are required. The charcoal, when it becomes exhausted of its decolorating properties, is again rendered *active* by being heated to dull redness in closed cylinders; this *restoring* costs about sixpence per cwt. of charcoal; and the quantity employed to decolorate a given quantity of "raw sugar" is at the rate of one cwt. to three cwt. of sugar.

The advantages this plan possesses over the old method of using charcoal in fine powder are—increased decolorating power and superior crystallisation. The disadvantages are,—the great quantity of charcoal required; the immense quantity of *sweet water* (thin solution of sugar) produced. This latter is indeed the principal objection against the plan, there being a great deal more "sweet water" made than can be got rid of in any other way than by evaporating it separately. Another disadvantage attending the "coarse grained" charcoal is the liability of spontaneous acidity taking place in the liquor, long before the charcoal is exhausted of its decolorating property; when this event occurs, the refiner has no remedy but getting out the liquor as speedily as possible.

It is exceedingly difficult to state accurately the produce of 112 pounds of raw sugar, as the refiners are most determinately silent on this subject. There can however be no doubt but that, even by the old plan, a greater weight of refined sugar can be obtained than the Government have founded their calculations in allowing drawback duty. The following weights are not far from the truth:—

	Old Plan.		*Howard's Plan.*
Refined	70 lbs.	Refined	79 lbs.
Bastard	22	Bastard	17
Treacle	20—15½ lbs. solid matter.	Treacle	16—12 lbs. solid matter.
Waste	4½	Waste	4
	112		112

In all descriptions of saccharine matter which assume the crystalline form, there exists a certain quantity of crystals or (in the language of the sugar refiner) *grain*, which quantity cannot by any process be increased, but may be diminished by the admixture of various foreign bodies. Several of the proximate vegetable principles, such as mucilage, gluten, tannic acid (tannin,) &c., diminish the production of crystals; all the acids produce similar results, so do all the alkalies, alkaline earths, and all those salts which, although neutral in composition, exert an acid reaction on test paper.

If a filtered solution of "raw" sugar is submitted to proper chemical tests, it will be found to contain mucilage, gluten, tannic acid, acetate of lime, chloride of sodium (muriate of soda,) oxide of iron, and saccharate of lime. That these bodies are foreign may be proved by taking some refined loaf sugar, dissolving it in distilled water, and then testing. On examination it will be found that none of the above bodies can be detected. That these bodies diminish the production of grain can be proved by two methods:—1st. If a solution of raw sugar be freed from them, its crystallising power will be found much increased. 2d. If a solution of refined sugar have these matters dissolved in it by artificial means, its crystallising power will be found materially injured.

All the foreign bodies, with the exception of acetate of lime and chloride of sodium, can be removed from "raw sugar." The salts formed by acetic acid and soda being soluble, all that could be done with these two bodies would be to change them into some other soluble compound; at the same time they do not injure the grain of the sugar, unless the quantity is enormously great. Hence, from the impossibility of making "grain" where it really does not exist, the utmost improvement which can be effected for "raw sugar" is to free it from these foreign bodies, which injure its graining power, leaving its natural grain at the same time uninjured.

We consider that the two following causes, hitherto entirely overlooked by practical, as by scientific, men, have been the grand obstacles to improvements in the manufacture:—1st. The affinity which sugar itself, when in solution, exerts on many chemical agents. 2d. The great specific gravity of the liquor which the refiner is obliged to employ; the sp. gr. being rarely under 1.280 at 60°, and frequently indicating a greater density.

The two following experiments will prove that great spec. grav. materially modifies chemical decomposition. 1st. Take a quantity of *liquor*, spec. grav. 1.280 at 60°, made from refined loaf sugar, and dissolve in it 20 grains of sulphate of protoxide of iron; now add and dissolve as much cream of lime, as would have perfectly decomposed the 20 grains of sulphate if water was the solvent. No precipitation takes place. 2d. If a quart of liquor made from similar sugar, but whose sp. gr. is only 1.020, had dissolved in it 20 grs. of the sulphate of protoxide of iron, and lime is now added in the same manner, immediate precipitation of sulphate of lime and protoxide of iron, ensue.

That sugar in solution is capable of modifying in a surprising manner well known chemical results, will be apparent from the following experiments:—1st. Let some liquor, sp. gr. 1.020, made from refined sugar, be boiled for a few minutes on a large excess of caustic lime, and then filtered; the liquor thus dissolving a large quantity of lime. Let oxalic acid be added to some of this caustic liquor, until it is slightly acid to the test paper; then filter, to separate insoluble oxalate of lime. If this acid liquor be again tested with oxalic acid, it gives a fresh precipitate. Hence liquor, even of the low sp. gr. of 1.020 can contain excess of lime, and excess of oxalic acid in the same solution, without acting on each other; yet oxalic acid is capable, if added in great excess, of precipitating the entire lime from a liquor of 1.300 sp. gr. 2d. If the foregoing experiment is reversed by adding oxalic acid in the first instance, and then attempting to neutralize by lime, it will be found that an excess of lime must be employed before the entire of the oxalic acid can be neutralised.

We have chosen oxalic acid and lime simply on account of the great affinity they manifest for each other, and the striking character they give to the experiments from the insoluble compounds formed. It will, however, be found, on examination, that all other acids, alkalies, or alkaline earths act on each other precisely in the same manner as above described, when *liquor* is the medium in which they are dissolved.

It may, perhaps, be as well to observe, that the foregoing remarks apply in the strictest manner to all salts, which when dissolved in *liquor* are attempted to be decomposed by the introduction of any alkali, or alkaline earth. Let sulphate of oxide of zinc, sulphate of protoxide of iron, ter-sulphate of peroxide of iron, sulphate or acetate of alumina, &c., be dissolved in "liquor," and the alkali, or alkaline earth, then added, no precipitation takes place; an excess of alkali, &c., must be employed, and this excess frequently redissolves the base of the salt as fast as it is decomposed. This redissolving arises from the power saccharine matter and alkalies, &c., have of forming soluble compounds with some of the oxides of the second class of metals, when neither saccharine matter, nor the alkalies, &c. separately have any such power. For instance, protoxide of iron is totally insoluble in "liquor," if such liquor is quite free from either acid or alkali, &c. A mixture of lime and water has no power whatever in dissolving protoxide of iron, but if some *liquor* saturated with lime is allowed, even when quite cold, to percolate through a bed of iron rust in coarse grain, 12 inches in depth, such liquor will issue from the iron as black as ink and completely loaded with protoxide of iron.

The same result attends the employment of sulphate of oxide of zinc. This salt has the property of precipitating vegetable colouring matter when in combination with tannic acid, and the latter is an impurity found in raw sugar. But the sulphate will be found to occasion only a very slight decoloration of the liquor. If lime be now added to decompose the sulphate, an excess must be used before decomposition takes place. This excess redissolves the oxide of zinc as fast as it is liberated: for lime and sugar, when in solution, act towards oxide of zinc as they do to protoxide of iron.

In the great sp. gr., &c., modifying affinity of *liquor*, we can trace the source of the many difficulties that have attended attempts in sugar refining. An examination of the properties of the different foreign bodies which can be removed from raw sugar, together with due estimation of the two foregoing causes, point out in what class of bodies to seek for an agent, or agents, capable of improving (if such a thing be possible,) sugar refining as now practised in great Britain.

Before attempting such an examination we may state some facts, which we believe to be new and of some value, connected with that plan (percolation through animal charcoal in a granular form) of refining sugar, which experience has pointed out as the most improved yet known.

[TO BE CONTINUED.]

Preparation of pure Antimony exempt from Arsenic and Iron. By JUSTUS LIEBIG.

The process proposed by M. Liebig is not expensive and may be executed with great facility, and it appears to have the advantage of removing at a single operation, arsenic, iron, and copper: only, if the regulus contain lead, it must be separated by other means. Antimony should be therefore taken free from lead; but if that cannot be procured, the latter metal must be first extracted from the sulphuret of antimony. For this purpose the method of Berthier is preferable: viz. melt together

100	Sulphuret of Antimony,
42	Iron,
10	Sulphate of Soda, dry, and
2	Charcoal.

From this we obtain from 60 to 62 of the regulus. This operation is easily performed without leveling. The scoriæ is very fluid, and the hammer easily separates it from the metal.

The following is Liebig's process:—16 parts of the commercial regulus of antimony are coarsely powdered and mixed with one part of sulphuret of antimony and two parts of dry carbonate of soda; melt the mixture in a Hessian crucible, and keep it an hour in fusion; let it cool, break the crucible and separate the scoriæ from the metal. Pound up the latter coarsely, mix it with one and a half parts of dry carbonate of soda, and keep it again an hour in fusion. Treat the metal a third time in the same manner with one part of carbonate of soda. The dross of the first melting is of a deep brown, that of the second is of a clear brown, of the third a clear, or almost of a citron, yellow, or a yellowish white.

After the third fusion the regulus is absolutely pure, and exempt from copper, arsenic and iron. It has a silvery whiteness and is very splendent. Slowly cooled, its fracture is fine granular,—rapidly cooled, it is in broad laminæ. Before the blowpipe on charcoal it melts into a globule with a pure, splendent surface. If a current of air from a blowpipe be directed on a red hot globule, the metal is volatilised, in a state of oxide, in dense white fumes, *completely inodorous*, and burns away entirely without recourse to the flame. If a globule of the melted metal be slowly cooled on charcoal, after being heated to redness, it becomes surrounded with a circle of pearly oxide in fine needles, *perfectly white, or rather of a pearly white.*

When deflagrated with nitre, it yields diaphoretic antimony of a brilliant white; if boiled with nitric acid, it furnishes no trace of iron or copper, a proof that the metal is sufficiently pure.

The impure regulus is immediately recognised by the manner in which it is affected by the blowpipe. It melts with rather greater difficulty on charcoal than the pure metal; the melted globule is soon covered by a dross of sulphuret of iron, or other metal; the surface becomes tarnished and the metal does not continue to burn when the flame is withdrawn. The coating of oxide on the charcoal is not white, but more or less yellow: the fumes have an odour of arsenic.

The addition of sulphuret of antimony is designed to convert all the arsenic, and part of the iron and copper, into sulphurets. By fusion with carbonate of soda, these sulphurets combine with the soda without leaving any metallic residue, and there is no sulphuret of antimony dissolved or transformed into liver of antimony, as long as there remains any non-oxidised arsenic in the regulus of antimony.

The mixture must not be left in fusion more than an hour, otherwise the carbonate of soda attacks and dissolves the matter of the crucible, and the risk is increased of losing some of the metal by the separation of the upper part of the crucible.

The dread which type-founders have of the fumes of their melted metal is well known. When breathed, the effect of the arsenic on the system is soon perceived, and the presence of this metal in the antimony of commerce is the principal cause of these accidents.

If the price of metal exempt from arsenic could be rendered unobjectionable, it is probable that it would be ever preferred,—for, in addition to considerations of health, compositions of the purified metal melt more kindly and agreeably, and the types are equally as hard, and much less liable to injury by the action of the air. G.

Jour. de Pharmacie.

Preparation of Iodine. By M. Bussy.

The process of extracting iodine generally followed, and which consists in decomposing the mother-waters of kelp by means of concentrated sulphuric acid, is well known to be liable to variable results, on account of a portion of the iodine coming over in the state of hydriodic acid and chloride of iodine, so that in either case there is always a considerable loss.

To avoid this inconvenience, M. Soubeiran proposed to precipitate the iodine from the mother-waters by means of sulphate of copper, and afterwards to decompose the iodide of copper by peroxide of manganese at a high temperature.* But this process requires minute attention and much precaution to separate the whole of the iodine existing in the mother-waters, and we do not believe that it has ever been employed in any manufactory.

These circumstances induce me to publish a far more simple method, which has been lately employed by some manufacturers of iodine; it was discovered (if we are rightly informed) by M. Barruel, director of the chemical works at the Faculty of Medicine; it consists in precipitating the iodine of the mother-waters of kelp by a current of chlorine.

In this process the mother-waters are first to be evaporated to dryness, and to the residue there is to be added a tenth of its weight of powdered peroxide of manganese; the two substances are to be perfectly mixed, and subjected to a dull red heat in an iron vessel, frequently stirring them; the object of this calcination is to convert the sulphurets and hyposulphates, of which the mother-waters contain a large quantity, into sulphates. It is extremely easy to determine when these compounds are converted into sulphates by taking a small quantity of the calcined matter and pouring upon it an excess of sulphuric acid. It ought not to yield, when the conversion is perfect, either sulphuretted hydrogen or a deposit of sulphur. If violet vapours are disengaged during the calcination, the heat must be lowered to avoid loss. When the sulphurets are decomposed, the residue is to be dissolved in a sufficient quantity of water to give a solution of sp. gr. 1.333; through this solution there is then to be passed a current of chlorine gas, taking care to stir it constantly with a glass rod: the liquor becomes of a deep brown colour, afterwards turbid, and deposits iodine in the form of a black powder; it is to be collected and distilled in a glass retort, in order to procure it in crystals. The only difficulty in this process is to determine the point at which the action of the chlorine should cease, that no excess may be used which would react upon the iodine precipitated. This excess of chlorine is especially to be apprehended, when it is also intended to separate from the same mother-waters the bromine which they contain.

In order to avoid adding excess of chlorine, the liquor which is supposed to be near saturation, may be suffered to settle for a short time, and the current of chlorine being interrupted, it is to be directed on the surface of the liquid. If it contain any iodine in solution, a pellicle of iodine will be observed to form on the surface; this effect is not produced when all the iodine is precipitated; in the latter case the liquor becomes quickly clear and retains only a reddish tint. Lon. & Ed Philos. Mag.

Preparation of Bromine. By M. Bussy.

The separation of bromine, as usually performed, also involves great difficulties, which may be avoided by the following process.

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* *Journ. de Pharmacie*, tom. ii. p. 411.

This process greatly resembles the preceding one; it is founded, like, it upon the great affinity of chlorine and the property which it possesses of displacing bromine from its combinations. It also includes the employment of the mother-waters of iodine, which without it are useless. Take the mother-waters of kelp, after having separated the iodine by chlorine as above described. These mother-waters contain a metallic bromide, when care has been taken to avoid the use of more chlorine than sufficient to precipitate all the iodine. Add to 1250 parts of these mother-waters, thirty-two parts of powdered peroxide of manganese, and twenty-four parts of sulphuric acid of sp. gr. 1.848: the whole is to be put into a tubulated glass retort, to which a tubulated receiver is to be adapted; and to this a tube which is immersed in a test tube. The retort and the receiver, as well as the receiver and the tube, ought to fit sufficiently well without the use of lute or corks, as they would be inevitably destroyed by the action of the chlorine. All being thus arranged, the retort is to be heated, so as to boil the liquid; the bromine condenses in the receiver in the form of red oily striæ, with a small quantity of water; the operation is to be discontinued when red vapours cease to be produced. On heating the receiver gently, without dismounting the apparatus, the bromine will pass into the test-tube, where it will condense on cooling.

The mother-waters which have been made use of may be rejected, on ascertaining, by the addition of a fresh quantity of sulphuric acid and peroxide of manganese that they contain no more bromine.—*Journal de Pharmacie*, January 1837. Ibid.

On the Extraction of Copper from Poor Ores, as practised successfully in Germany.

This process was first suggested by M. Karsten's observations in his "System of Metallurgy," in which he says:—"Very poor ores of oxydized copper, which could not be brought with advantage to the furnace, may be *roasted*, if mixed with pyrites of iron, or submitted in any way to the action of sulphuric acid gas, and will then form sulphates under the influence of the humidity of the atmosphere, or with water. Another method of extracting from similar ores, or even from the refuse, or scoria, is employed at Rheinbreitenback; and consists in mixing the old scoria of copper with old ores, and placing them in frames or boxes with numerous small holes in the bottom. A mixture of nitrous and sulphurous gas is injected into the centre of these boxes, with steam at certain intervals. As soon as the sulphuric acid is formed, it acts upon the metallic oxides by producing vitriol and copperas; besides which, the sulphuric acid acting only on the oxide of copper, a *sulphite* of this metal arises, which is speedily transformed into a sulphate, and the metallic salts being separated in the usual manner, they are again concentrated, and the copper is precipitated with heat by means of old iron; the copper thus obtained is afterwards mixed with flour of sulphur, then heated in a reverberating furnace; the result is the formation of sulphate of copper, which being dissolved with water and crystallized, is fit for the purposes of commerce. The liquid from which this copper is procured, is used for obtaining sulphate of iron."—*Annales des Mines.* Mining Jour.

Whiting's Patent Sashes.*

Your correspondent A. W. S. very justly complains of the annoyance occasioned by the rattling of the sashes, which prevails more or less in most houses with sash windows; in the majority of these cases, the cause I take to be (with all due deference to the opinion of Mr. Medcalf. Vol. III. p. 238,) the inferiority of the materials of which they and the frames are composed. I have seen a specimen of "*Whiting's patent sashes and frame*" at the house of his agent here, which appear to me to be well adapted for avoiding the above-named annoyance, and require only to be better known to insure their more extensive use. The frames are of solid deal, in which are square sinkings for the reception of the leaden weights; over these are fixed beaded linings, which also conceal the lines, and form small grooves to receive the metal tongues, fixed in the styles of the sashes: a metal tongue is fixed in the head, and another in the sill of the frame, catching the top and bottom rails when the sashes are shut; the meetings are beveled, and fastened with a common brass sash-fastener. The whole forming a very compact air and water tight window. Arch. Mag.

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* ——— Whiting, Esq., architect and county surveyor, Ipswich.

A Plan by which the Lines, &c., of hung Window-shutters may be concealed.

A friend of mine, a few years back, executed a plan which perfectly succeeded in producing the above effect. The shutters are made in two heights, hung as mentioned by Eboracum; and they slide down into a case, the side of which is formed by the window front. This case is closed at top by a flap, hinged on the outer edge, which conceals the shutters when they are down, and supports them when they are up. There are similar flaps to cover the pulley-styles and lines; but they are hinged on the inner edge, and they unite with the architrave, and effectually hide the lines, and keep the first-mentioned flap in its place when the shutters are down. These flaps are thrown back, when the shutters are up, between them and the sash bead of the window; sufficient space to allow of this, being left between it and the inner edge of the horizontal top. Ibid.

Indelible Inks.

No. 1.—For writing with quill-pens.

Indian ink, dissolved in water acidulated with hydrochloric[*] acid of commerce, and marking 1½° on Beaumé's areometer.[†]

No. 2.—For writing with metallic pens.

Indian ink, dissolved in water rendered alkaline by caustic soda, and marking 1° on Beaumé's areometer.

No. 3.—For printing the indelible designs and patterns.

Common printing-ink, whitened with a sufficient quantity of sulphate of barytes, either artificial or natural, which has been ground in water for a considerable time. Mag. Pop. Sci.

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[*] Muriatic acid, about sp. gr. 1.28, so called by the French chemists.

[†] As Beaumé's areometer is but little used in this country, it may be useful to state that if water be taken at 1000 sp. gr.

1°	on this instrument is . .	1007 sp. gr.
1½°	ditto	1010 "
2°	ditto . . .	1014 "

For the acidulated solution.—To a pint of water add about three teaspoonfuls of muriatic acid.

For the alkalized solution.—To the same quantity of water add 100 grains of pure

Iron from Anthracite.

In Mr. Byer's lecture, on Galvanism, when under the head of Electro-magnetism, he exhibited a bar of cast-iron, eighteen inches long, and about half an inch square, which was made at the Yniscedwyn Iron Works, by the hot blast with anthracite. This bar, previous to the lecture, Mr. Byers placed within a helix of coated copper wire, about 100 feet long, and through which he passed two shocks from a small battery; the bar was subject to the action of the battery about two or three seconds, and when withdrawn from the helix, it was found to be permanently magnetised, exactly as if it had been a bar of cast-steel. Mr. B. stated he was not aware that *cast-iron* was affected in this manner, but it was known that common wrought-iron would not retain a charge of electricity so as to become permanently magnetic. Mr. B. therefore, concludes *this* iron (as now manufactured at Yniscedwyn) is similar to cast-steel, as far as its magnetic properties are considered.—*Merthyr Guardian.* Mining Journal.

Stone Coal Iron.

What we call stone coal, (the learned name anthracite) from the Greek word anthrax, which means charcoal. We understand that a short account is about to be published respecting the newly invented mode of manufacturing iron with that coal; in the mean time we can inform our readers, on good authority, that from repeated experiments made in the presence of numerous persons, it is found that a rail of a certain size, made in the old process, breaks under the weight of twelve tons, and a rail of precisely the same shape and dimensions, made of stone coal iron, broke under fourteen tons and a half; this is of importance in railroads. The stone coal iron is extremely pure, devoid of phosphorus, sulphur, and all other obnoxious substances, and bears a close resemblance, in quality, to foreign iron made of charcoal. One ton of iron is produced from a ton and a half of stone coal in the furnace, and eight hundred weight of small refuse coal for the heating apparatus, whereas two and a half tons of bituminous coal, are required to make a ton of pig-iron, and seven tons to make a ton of bar-iron. Of course the quantity varies according to the quality of the coal; but, the above will be found a fair average.—*Merthyr Guardian.* Ibid.

Present amount of Steam-power in Birmingham.

The number of steam-engines actually working at the present time in Birmingham, is *one hundred and sixty-nine*, amounting in the aggregate to

caustic soda, of the kind prepared for chemical purposes.

About 40 grains of Indian ink may be dissolved in a pint of the solution. This will make an ink sufficiently black for ordinary writing, but it is evident it may be very easily made darker, or lighter, at pleasure.

The Indian ink should be of the best quality. Care should be taken that it be uniformly good throughout the stick.

An inferior kind, hard, gritty, and possessed of little colouring power, is sometimes imposed upon the purchaser, by the deceptive practice of attaching a little bit of good ink at each end of the stick. A good mode of examination is to break the stick in two: this defeats the above trick, and exposes the fracture; that of good ink is brilliant, angular, and clean; the bad, dull and earthy.

two thousand seven hundred horses-power. They are distributed as follows:

	Horses-power.
In grinding flour	275
— working metals	1770
— pumping water	279
— glass-grinding	87
— working wood	97
— making and glazing paper	44
— grinding clay	37
— grinding colours and chemicals	61
— sundries	50
	2700

[*Birmingham Philosophical Institution. Report of Committee*, 1836.]

Mag. Pop. Science.

Progress of Physical Science.

On the Physical Causes of the principal Phenomena of Heat.

By John Barton, *Esq.*

It has always appeared to me that the corpuscular hypothesis is capable, with some modification, of affording a more complete and satisfactory explanation of the phonomena of heat and light than the undulatory hypothesis. On the present occasion I propose, without entering into any controversial discussion, to show in what manner the principal phenomena of heat may be deduced from the action of two forces. An attractive force between the particles of heat and those of solid matter, and a repulsive force between the particles of heat themselves.

I assume that the particles of heat are very small in comparison of the particles of solid matter, and that these last are very small in comparison of the intervals by which they are separated from each other. The phenomena also require that the repulsive force should decrease more rapidly than the attractive force. These premises being admitted, the following consequences may, if I do not mistake, be deduced from them. For brevity's sake I omit the demonstrations, presuming that they will be readily supplied by those versed in mathematics.

1. If a particle of heat approach a particle of solid matter, it will either fall to the surface and remain there, or will describe a curvilinear orbit thereabout. According to the direction and velocity of its approach, this orbit will either be confined within a certain limit, like an ellipse, or will go off to an infinite distance, like an hyperbola. To avoid circumlocution, I will take leave to call these two classes of curves respectively, *ellipsoidal* and *hyperboloidal* curves. A particle of heat reposing on the surface of a particle of solid matter, or revolving about it in an ellipsoidal curve, will have no tendency to fly off, or to pass into a contiguous body, unless disturbed by some interior force. It is therefore *latent.*

It must not be supposed that latent heat exists only in fluid or gaseous bodies. The phænomena of softness and malleability must be considered as resulting from the presence of a portion of heat in this state. Nay, it is probable that even the hardest and most brittle substances are not entirely deprived of it.

2. When a solid body is exposed to friction, or to percussion, a certain number of the particles of heat which had revolved tranquilly in ellipsoidal curves are forced out of their orbits, and fly off beyond the reach of the attractive force of the particle to which they belonged; just as our earth might be driven out of the limits of the solar system by the stroke of a comet; supposing the latter of sufficient density and magnitude. And thus we have an explanation of the heat produced by friction, or percussion; which has been supposed to form one of the strongest objections to the corpuscular hypothesis.

It confirms this theory, that a piece of iron which has been heated by hammering becomes *brittle*; indicating that a portion of its latent heat has been lost, while sensible heat has been disengaged.

3. If we suppose a particle of matter to be of an oblong form, or to have one of its axes greater than the others, the particles of heat will collect chiefly about the middle of its length. And thus we have an explanation of a remarkable fact discovered by M. Mitscherlich, that crystalized bodies, when heated, do not expand equally in all directions.*

4. When the coefficients of the attractive and repulsive forces bear a certain relation to each other, and to the original velocity of approach, the orbit described will resemble a *conchoid*, its two branches being ultimately in one and the same line. In this case the body is *diathermanous* the rays of heat passing through it apparently in a straight course. The course of a ray through a solid body can never in fact be straight, unless its velocity be infinite; but in proportion as the velocity approaches this limit, the ray will experience less disturbance in passing through bodies imperfectly diathermanous. And accordingly it is found, that rays from incandescent bodies pass through some media which do not admit the passage of rays issuing from bodies of lower temperature, the velocity of projection evidently depending on the repulsive force; and this, in its turn on the temperature.

5. It must not however be supposed that opaque or *adiathermanous* bodies are impervious to the rays of heat. They differ from transparent or diathermanous bodies only in this, that the entering ray, instead of passing through immediately, and escaping from the opposite side, is entangled in a circuitous and irregular course among the particles, until by chance it reaches the surface, when it escapes, unless the angle of its direction with the tangent be so small that it is drawn back again by the attractive force. A particle of heat is therefore detained much longer in passing through an opaque than through a transparent body, and contributes much more to raise its temperature, and hence it also appears why the passage of heat through opaque bodies is very much slower than through transparent media.

6. The particles of solid bodies are drawn towards each other by two forces, their mutual attraction, and their attraction for the atmospheres of heat about the others. They are also kept apart by two forces, the mutual repulsion of those atmospheres of heat, and the repulsion of the particles of free or sensible heat, which happen at the time to be passing between them. As long as the number of these last remains unchanged, the balance of the opposing forces is a balance of stable equilibrium, in as much as the attractive force diminishes with the distance in a less ratio than the repulsive force.

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* See Phil. Mag., First Series, vol. lxiv. p. 162; and Lond. and Edinb. Phil. Mag. vol. i. p. 413.—Edit.

If the number of particles of sensible heat is increased, the body dilates till the equilibrium is restored; but if this dilatation proceed to a certain point, the orbits of a certain number of the revolving particle of heat will be changed from the *hyperboloidal* to the *ellipsoidal* form; that is to say, a certain quantity of sensible heat will be converted into latent heat. At the same time another portion of sensible heat will be acquired from the surrounding bodies and the united repulsive force of these two portions of heat will overbalance the forces of attraction. Thus we have an explanation of the phenomena of *vaporization.*

An account of the causes of this last class of phenomena somewhat resembling the above has been given by Laplace in the twelfth book of the *Mecanique Celeste.* Laplace however gives no explanation of the difference between latent and sensible heat: he attributes the phenomena of expansion exclusively to sensible heat, but does not explain why heat when it becomes latent should lose its repulsive force: he supposes the particles of heat to form atmospheres about the particles of solid matter, which are at rest unless disturbed; and in order to explain why sensible heat tends continually to fly off, he supposes the particles of matter to be in a state of continual agitation.

7. In the same chapter Laplace has given an explanation of the phenomena of liquefaction, which appears to me to require modification. "Every molecule of solid matter," he says, "is subjected to the action of three forces: 1st, the attraction of the surrounding molecules; 2ndly, the attraction of the caloric of those molecules. *plus* their attraction for its caloric; 3dly, the repulsion of its caloric by the caloric of those molecules. The first two forces tend to bring the molecules nearer together, the third to separate them. The three states of *solidity*, *fluidity*, and *gaseous elasticity* depend on the respective efficacy of those forces. In the state of solidity, the first force is the most powerful; the influence of the figure of the molecules is very considerable, and they are united in the direction of their greatest attraction. The increase of caloric lessens this influence by dilating the body, and when this increase is such that the influence in question is very small, or none, the second force predominates, and the body takes the liquid form."

I am unable to comprehend why the attractive force between two solid particles should be suddenly reduced to nothing when they are separated to a certain distance one from the other. And this suggestion appears still less probable when it is considered that many crystalizable bodies cease to contract, and even undergo a degree of expansion, at the moment of congelation. The act of changing from fluid to solid depends then on some other cause than the approximation of the particles. Laplace has, I think, truly assigned the immediate physical cause of solidity. when he says that in solid bodies the particles are placed relatively to each other in the position of greatest attraction; but he has not given an adequate reason for their assuming this position. To supply this deficiency, it may be useful to revert to an explanation above given of an observation of Mitscherlich on the unequal expansion of crystalized bodies in different directions. It was suggested that the particles of heat do not accumulate equally round every part of a particle of solid matter if its axes are of unequal length. Now, this inequality of distribution is such, by the nature of the case, as to counteract the tendency of the particles to place themselves in the position of greatest attraction. In other words, the compound force exerted by the central solid nucleus, with its surrounding atmosphere of heat, will approach more and more nearly to that of a sphere as the heat increases; and by consequence the figure of the particles exercises a less and less influence on their mutual position. When that influence is completely neutralized, the body is a fluid.

8. As the mutual attraction of the particles is not however destroyed in fluids, these particles still arrange themselves in the position of greatest attraction. But this position is no longer influenced by their figure; it is now such, precisely, as would be assumed by them if spherical, that is to say, the position in which the distance of their centres is a minimum. Now this is the position in which the sum of the intensities between them is also a minimum; therefore the whole bulk of the fluid just before congelation is a minimum. If we suppose other forces to come into operation, those, for instance, resulting from the figure of the particles, the dimensions of the body will consequently be enlarged; and thus it appears why so many substances are found to expand in the act of congelation.

9. Further, as the position of greatest attraction amongst the particles of a solid is that in which their salient angles are presented towards each other, it follows that at the moment of solidification, when this arrangement is established, the orbits of a certain portion of the accompanying particles of heat will be changed from the *ellipsoidal* to the *hyporboloidal* form; that is to say, a certain quantity of latent heat is converted into sensible heat.

10. Those solid bodies which are composed of the largest, or rather the heaviest, particles, are at once the best conductors, and the worst radiators, of heat; for the velocity increases with the mass. Hence the metals are the best conductors of heat, and as far as the experiments hitherto made enable us to judge, their conducting power seems to follow the same order with the magnitude of their component atoms, the differences not being greater than may be reasonably supposed to arise from errors of observation. For a similar reason, a particle of heat attempting to escape from the surface of a metallic body is more strongly drawn back by the attractive force of the particles than at the surface of other bodies. The metals have therefore less radiating power than any other substance.

11. The particles of heat emerging from the surfaces of solid bodies at angles so small as to be drawn back again into their substance, form collectively an atmosphere enveloping those surfaces, and extending, there is reason to believe, to distances considerably greater than the interval which separates the solid particles from one another. It is to this atmosphere that we must attribute the repulsive power exerted at the surfaces of bodies, metallic bodies especially, by virtue of which mercury is depressed in a barometer tube, and a steel needle floats on the surface of water. By strong pressure this enveloping atmosphere may be expelled from between two metallic surfaces, and the attractive force of their particles then coming into play, they adhere with considerable force. The reflection of heat at the surfaces of bodies also appears to be due to the action of this enveloping atmosphere.

12. It may facilitate the apprehension of my meaning to observe, that a particle of matter, with its revolving particles of heat, is supposed to have a resemblance to the sun, with its accompanying bodies, in the solar system. The planets, revolving in elliptic orbits, represent the particles of latent heat; the comets, if indeed any of them revolve, as formerly supposed, in parabolic or hyperbolic orbits, represent the particles of sensible heat. Future observations will, perhaps, enable us to determine the law of the forces by which these minute movements are regulated, as accurately as we are

now acquainted with the law of gravitation. The preceding conclusions are, however, independent of the particular law of those forces.

January, 27, 1837.

P. S. I have assumed, in conformity with the views of Laplace and other mathematicians, that the particles of solid matter mutually attract each other. But the preceding conclusions hold good though no such force of attraction exist, or even if we suppose, with Æpinus, that the particles mutually *repel* each other, a supposition which is by no means incompatible, as Dr. Roget has observed,* with the phenomena of gravitation. There are certainly strong reasons in support of this last hypothesis.

Lon. & Ed. Philos. Mag

Simultaneous Meteorology.

The value of the plan of observation suggested in the following paper, coming as it probably does, from one of the most distinguished philosophers of the present day, will be acknowledged, we trust, by every reader; and we cannot doubt that in various parts of the United States, observers will be found, who, as far as their means extend, will carefully practice upon these suggestions. If no central point for the reception of the Registers made in this country, should occur, preferable to the Franklin Institute, they will be cordially received by the Actuary, and properly disposed of by a committee of the Institute. Independently of the benefit which such simultaneous American observations may confer upon Science by being forwarded to the Cape of Good Hope, they will have a high value in reference to the progress of meteorological knowledge among ourselves, and come into admirable unison with the encouragement now afforded by some of our state governments, (and which we hope to see adopted in time by all of them) for the procuring of regular and accurate Registers of observation throughout their limits. Hoping to receive from more places than one in the United States a series of observations, made at the time of Equinoxes and Solstices as recommended in this paper, and upon the plan of those given in page 192, we shall continue to insert, from the foreign Journals, one table of the hourly observations made in England at those times, for the purpose of comparison. G.

In some "Instructions for making and registering Meteorological Observations," circulated by the South African Literary and Philosophical Institution, established at the Cape of Good Hope, and drawn up, it is believed, by Sir John Herschel, there is, among others, the following recommendation :—

"With a view to the better determining the laws of the diurnal changes taking place in the atmosphere, and to the obtaining a knowledge of the correspondence of its movements and affections over great regions of the earth's surface, or even over the whole globe, four days in each year should henceforward be especially set apart by meteorologists in every part of the world, and devoted to most scrupulous and accurate registry of the state of the barometer and thermometer ; the direction and force of the wind ; the quantity, character, distribution of clouds ; and every other particular of weather, throughout the whole twenty-four hours of those days, and the adjoining six hours of the days preceding and following.† The days

*See Scientific Memoirs, Part III. p. 469.—Edit.

†This is necessary by reason of the want of coincidence of *the day* in different parts of the globe, arising from difference of longitude. In order to obtain a complete correspondence of observation for twenty-four successive hours over the whole globe,

recommended for these observations are, the 21st of March, the 21st of June, the 21st of September, and the 21st of December, being those, or immediately adjoining to those, of the equinoxes and solstices, in which the solar influence is either stationary, or in a state of most rapid variation. *But should any one of those 21st days fall on Sunday, then it will be understood that the observations are to be deferred till the next day, the 22nd.* The observation at each station should commence at 6 o'clock A. M. of the appointed days, and terminate at 6 o'clock P. M. of the days following according to the usual reckoning of time at the place. During this interval, the barometer and thermometer should be read off, and registered hourly, and the precise hour *and minute* of each reading should be especially noted.

"For obvious reasons, however, the commencement of every hour should if practicable, be chosen, and every such series of observations should be accompanied by a notice of the means used to obtain the time, and, when practicable, by some observation of an astronomical nature, by which the time can be independently ascertained within a minute or two.* As there is scarcely any class of observations by which meteorology can be more extensively and essentially promoted, it is hoped that not only at every station of importance in this colony, but over the whole world, and on board ships in every part of the ocean, individuals will be found to co-operate in this inquiry. Every communication of such observations, addressed by channels as secure and as little expensive as possible, to the Secretary of this Institution, will be considered as highly valuable."

In cheerful compliance with the above valuable recommendation, the observer to whom is confided the formation of our Meteorological Table, undertook the proposed series of observations during the period which has lately passed. The results will be found in the Table which follows, and which we transmit to the Secretary of the Institution at the Cape, as our contribution to the collection which is there proposed to be amassed and concentrated from the whole surface of the globe.

Observations of the same nature, and at the same times, have been taken at the Royal Observatory, Greenwich; at the apartments of the Royal Society, Somerset House; at the Observatory of Cambridge, &c.

As one of the most important ends of meteorological observations is their comparison with each other, and as, for this purpose, it is very desirable that observers should understand the use and adjustment of the instruments in their possession, we have been induced to select from these instructions such of them as are of general application.

I.—*General Recommendations and Precautions.*

"1. The continuity of observations ought to be interrupted as little as possible by changes in the adjustments of instruments—in their places—exposure—mode of fixing—or of reading off and registering them. Whenever any alteration in these, or any other particulars, takes place, especially such as are likely to effect the zero points, or otherwise to influence the mean results, it should be noticed in the register.

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it must be taken into account that opposite longitudes differ twelve hours in their reckoning of time. By the arrangement in the text, the whole of the *astronomical day*, (from noon to noon) is embraced in each series, and no observer is required to watch two nights in succession.

* For example, the first appea ances and last disappearances of the sun's upper and lower border, above and below the sea-horizon, if at sea or on the coast,—or on land the exact length of the shadow of a vertical object of a determinate length on an horizontal level, at a precise moment of time (not too near noon,) &c.

TABLE of HOURLY METEOROLOGICAL OBSERVATIONS, made during the 21st and 22d of March, 1837, at BLACKHEATH ROAD, near Greenwich, about four and a-half miles to the south-east of London, by J. H. BELVILLE, in conformity with the Instruction circulated by the South African Literary and Philosophical Institution.

DAY.	HOUR.	BARO-METER.	THERM.		WIND.		CLOUDS, &c.
			Attach'd	In open air.	Direction.	Strength	
		English Inches.	F. °	Fah. °			
Tuesday, March 21, 1837.	VI. A. M.	29.971	42	24.2	N. by W.	1	Snow on the ground. Intense frost. A few clouds to N. A Radiator on the ground had fallen to 19°
	VII.	29.967	42	26.8	N.	1	Clouds coming up. Sunshine through thin clouds.
	VIII.	29.962	39	28.2	N.	1	Clouds collecting, and overspreading the sky. *Cirro-stratus.*
	IX.	29.943	36	29.6	N.	1	Clouds increasing. Snow coming on from the northward.
	X.	29.928	35	30.0	N.	1	Snowing. *Cirro-stratus.*
	XI.	29.903	35	29.0	N. N. W.	2	Small snow. [*muli.*
	XII.	29.892	36	31.2	N.	1	Clouds broken. *Cirro-stratus* into *cu-*
	I. P. M.	29.853	36	31.1	N.	1	Clouds broken. Snow still falling.
	II.	29.848	37	31.5	N.	1	A shower of granular snow has just fallen.
	III.	29.843	37	33.0	N.	1	Large dense *cumuli.* Clear sky be.
	IV.	29.836	39	33.4	N. E.	1	*Nimbus* advances from N. [tween-
	V.	29.839	38	31.5	E.	0	Shower of snow. Wind changes to E.
	VI.	29.841	39	29.8	E. S. E.	1	Snow, beautifully crystallized of a *hexagonal* form, falls, and remains on the ground.
	VII.	29.839	38	28.9	S. E.	1	Clearer. Moon visible.
	VIII.	29.838	42	28.7	S. E.	1	Clouds again collect from the N.
	IX.	29.830	39	28.6	S. S. E.	0	Getting quite overcast. Very cold.
	X.	29.830	42	28.4	S.	0	Generally cloudy, and calm.
	XI.	29.824	43	28.8	S.	0	The same.
	XII.	29.818	43	28.7	S. W.	0	The same.
Wednesday, March 22.	I. A. M.	29.848	48	28.7	S.	0	The *cirro-stratus* clouds prevail.
	II.	29.797	43	28.7	S.	1	Sky quite covered, as before.
	III.	29.798	43	28.6	S.	0	The same, Night extremely cold.
	IV.	29.796	43	28.6	S.	0	The same. ground hard with frost.
	V.	29.796	42	28.0	S. W.	0	The same. Ice nearly an inch thick
	VI.	29.798	42	27.9	S.	0	Clouds broken to the east. Sun visible at his rising. [quickly from N.
	VII.	29 81 4	42	28.5	S. E.	1	Clouds getting thinner, and moving
	VIII.	29.836	42	29.1	E. S. E.	1	Getting clear, particularly to the N. E.
	IX.	29.838	39	31.8	E.	1	Still clearer. Sun Shining bright.
	X.	29.839	40	34.0	E.	1	Fine. *Cumuli* at a distance.
	XI.	29.842	41	36 1	E.	2	The same.
	XII.	29.858	42	38.4	S. E.	1	*Cumuli* collect apace.
	I. P. M	29.868	43	37.7	S. E.	1	More overcast. [snow fall.
	II.	29.864	43	37.0	E. S. E.	1	*Cumuli* into *cum.-stratus.* Flakes of
	III.	29.866	43	36.8	S. E.	0	Thick. No blue sky visible.
	IV.	29.866	42	36.1	E.	0	The same. Upper atmosphere hazy.
	V.	29.870	42	33.0	E.	2	Clouds thinner in zenith. Air feels cuttingly cold.
	VI.	29.904	42	31.5	E.	1	Getting quite cloudy and thick.

NOTES.—The time was taken from a good clock, keeping nearly mean time, its rate being scarcely perceptible, and its error obtained by observation of the Ball-Drop of the Royal Observatory, Greenwich.

The Barometer has an elevation of about forty feet above the River Thames.—Ma-

"1. So far as possible, registers should be complete—but if, by unavoidable circumstances of absence, or from other causes, blanks occur, no attempt to fill them up by general recollection, or by the apparent course of the numbers before and after, should ever be made.

"3. The observations should, if possible, all be made by one person—but as this may often be impracticable, the principal observer should take care to instruct one or more of his family how to do it, and should satisfy himself by many trials that they observe alike.

"4. The entries in the register should be made at the time of observation, and the numbers entered should be those actually read off on the respective scales of each instrument, on no account applying to them previous to entry *any sort of correction, as, for instance, for zero, for temperature, capillarity, &c.* All these and the like corrections, being matter of calculation and reasoning from other observations, are to be reserved till the final discussion of the series, and for separate determination and statement.*

"5. If copies be taken of the registers, they should be carefully compared with the originals by two persons, one reading aloud from the original, and the other attending to the copy, and then exchanging parts, a process always advisable wherever great masses of figures are required to be correctly copied.

"6. The register of every instrument should be kept in parts of its own scale, as read off, no reduction of foreign measures, or degrees, to British being made—but it should of course be stated *what* scale is used in each instrument.

II.—*Of the Times of Observation and Registry.*

"Meteorological observations should be made and registered daily, at stated and regular hours. In fixing on these, some sacrifice of system must of necessity be made to the convenience and habits of the observer. The best hours in a scientific point of view, would be those of Sun-rise, Noon Sun-set, and Midnight. But these are not the hours adapted to general habits; and since the midnight observation is likely to be pretty generally neglected elsewhere than in an Astronomical Observatory, the following hours, for a division of the day into three parts, are proposed for what may be deemed the Morning, Afternoon, and Evening observations, viz:

Morning . . .	8 A. M.
Afternoon . . .	2 P. M.
Evening . . .	8 P. M.

"If, however, the habits or engagements of any one should not allow him to conform to these hours, rather than not observe he may select his own, specifying only what they are at the head of every page of his register, and adhering steadily to them in practice, only observing to make the extreme observations of each day equidistant from the middle one.

"At the same time it will be borne in mind, that, in what concerns great meteorological questions on which the most interesting features of the subject depend, the night is quite as important as the day, and has

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kers, Watkins and Hill, London.——The Thermometer has a northern aspect.

The strength of the wind is indicated by the figures in the column, thus—0 marks that there was *no wind perceptible*; 1, *a very light breeze*; 2, *a strong breeze.*

*We regard this as of the highest importance.

hitherto been far too much neglected. To any one, therefore, who may feel disposed to enter more zealously into the subject, and will not consider some personal inconvenience ill undergone for the sake of affording data of a peculiarly valuable description, this Committee would most earnestly recommend the adoption, in preference to all others, of the quaternary division of the 24 hours.

III.—*Of Meteorological Instruments,—and, first, of the Barometer and its attached Thermometer.*

"The Barometer is the most important of all meteorological instruments. Its office is to measure the actual pressure of the atmosphere on a given horizontal surface at the time and place of observation. Its fluctuations are observed to have considerable relation to changes in the weather, and especially of the wind. Hence its use as a weatherglass.

"A barometer should be examined, before setting it up, for air-bubbles in the tube, and for the existence of air above the mercury in the upper part of the tube. This is done by gently inclining the instrument either way from the horizontal position a little up and down; when air-bubbles, if large, will be seen to run to and fro, and must be evacuated by inverting the instrument, and by gentle blows on it with the hand, driving them up into the cistern. If this cannot be done, the instrument is useless. If air exists to an objectionable amount *above* the quicksilver, it will not tap *sharp* against the upper end of the tube, when the barometer is quickly inclined from a vertical position, so as to make the mercury rise above its level, nearly to the top, and then gently *jerked* lengthways and backwards. If the blow is buffy and dead, or is not heard at all, the amount of air must be considerable, and may be expelled by inversion,

"In fixing the barometer, choose a good light near a window, but not exposed to sunshine, in a retired apartment little liable to sudden changes of temperature or to draughts of wind. Adjust the table to a vertical position by a plumb-line, and fix it so as never to shift from that position. Before reading off, give a few taps, on the instrument, enough to make the upper end of the column of silver *shake* visibly, as the mercury is apt to adhere to the glass, and give erroneous readings. In reading, bring the index always opposite to one part. The correct part to choose is the summit of the convexity of the mercury, to which the index should be made a tangent; but if this be difficult to hit, either from the construction of the index or the want of a proper fall of light, the line of junction of the mercury and glass may be taken. In that case, the tapping should never be omitted. Which ever mode of reading is once adopted, should be stated, and always adhered to. A piece of white paper placed behind the upper part of the tube will generally enable any one to read off by the convexity of the quicksilver. In placing the index, notice whether it appears to shift a little up and down as the eye is raised or depressed: this is called parallax, and is a source of uncertainty, to be avoided by placing the eye in reading always on the *exact level* of the top of the mercurial column.

"Barometric observations require corrections of three kinds; and to render them available and comparable with others, it is necessary that their amount should be ascertained, and distinctly stated. The first is called the Zero Correction. It includes several subordinate corrections arising from different sources, such as that originating in a faulty placing of the scale of inches, that due to the capillary depression of the mercury in the glass-tube, and the constant part (which at a fixed station is nearly the whole) of the depression arising from the presence of air or vapour in the upper part of the tube.

"To determine the zero correction, the barometer must be compared with a standard instrument, such as that at the Royal Observatory for instance, or some other which has been compared with it, or with some standard of equal authority. Such comparison ought never to be omitted before forwarding the barometer to its place of destination, nor should any opportunity be neglected of comparing it, when fixed in its place, with a good portable barometer. In making such comparisons, all that is necessary is to record the readings of both the instruments, after, at least, an hour's quiet exposure, side by side, that they may have the same temperature. If compared by two observers, each should read off his own barometer in his usual manner, and each should take a mean of several readings, then each should verify the other's results. By this means the zero of one standard may be transported over all the world, and that of all others compared with it, ascertained.

"The amount of the zero correction is often very large, as two or three tenths of an inch, but its influence on the mean results of recorded observations, falls wholly on the determination of the heights of the station of observation above the mean level of the sea, and effects little, if at all, any conclusions of a meteorological nature which may be deduced from them. Hence, if proper care be taken to preserve a barometer, once set up, immovable, a long and regular series of observation with it has a value independent of any knowledge of this element, and it is fortunate that this is the case, as the zero correction is one extremely difficult to determine exactly *a priori*.

"In transporting a *compared* barometer to its place of destination, great care is necessary. It should always be carried *upright*, or considerably inclined, and *inverted*, and over all rough roads should be carried in the hand, to break the shocks to which it would otherwise be exposed. Strapped obliquely across the shoulder of a horseman, however, it travels securely and well, and with common care in this mode of transport, its zero runs no risk of change.

"The next correction, and the most important of all, is that due to the temperature of the mercury in the barometer-tube at the time of observation. To obtain this, every barometer requires to have attached to, or fixed very near it, a thermometer, called the attached thermometer, which must be read and registered at each observation of the barometer. It is preferable in practice to read off this thermometer *first* to avoid the error arising from breathing on, or standing long near it, while reading the barometer itself. The zero of this thermometer should be ascertained by comparison with a standard at the temperature of about 60° Fahr.

"The third correction applicable to barometric observations arises from change of level of the mercurial surface in the cistern, owing to the transfer of a portion of its contents to, or from, the tube. In barometers with small cisterns, and where the lower level cannot be adjusted at each observation, its amount may be large, and its effect being always to make the apparent fluctuation less than the real, *in a fixed proportion*, it ought, if possible, to be ascertained. The data necessary to be known are—first, the internal and external diameters of the tube—secondly, that of the cistern containing the mercury, at the surface, where the tube plunges into it. These particulars, as they must be known to the maker, ought be inquired of him, and indeed ought to be engraved conspicuously on some part of the instrument.

"Although all these corrections are necessary for the strict *reduction* of registered observations, they ought not to be applied to individual observations previous to registry. It is sufficient to know them. Their effect is in all cases easily and safely applicable to mean results, and to the conclusions therefrom deduced, and a world of troublesome, and often mistaken, calculations may be saved by so applying them

"*Of the External Thermometer.*—The external thermometer should have a scale on which whole degrees are read off, and divisions large enough to admit of estimating tenths, or at least quarters of degrees, by the eye. It should be compared with a standard, and the difference stated, at one or more temperatures, (the wider asunder the better) within the range of the climate in which it is to be used. In fixing it, choose a perfectly shaded, but otherwise free, exposure, and one where no *reflected* sunbeams from water, buildings, rocks, or dry soil, can reach it, and easily accessible for reading. There fix it firmly and upright. In reading it, avoid touching, breathing on, or in any way warming it, by near approach of the person. The quicker the reading is done the better.

"Although read off at stated times, notice should be taken of all sudden and remarkable changes of temperature, as indicated by the external thermometer, whenever they occur.

"*Of the Maximum and Minimum, Self-registering Thermometer.*—This should be placed horizontally in some place out of doors, shaded from direct radiation and rain, and otherwise freely exposed to air, and so fastened as to allow of one end being detached from the fastening and lifted up, so as to let the indexes within the boxes slide down to the ends of the fluid columns; a more convenient mode, when the steel index is free enough to allow it, than the use of a magnet.

"Both the thermometers should be read off as early as possible every morning, and the indexes re-adjusted. But as double maxima frequently, and occasionally double minima, occur, in consequence of sudden changes of temperature, it is recommended occasionally to inspect both of them, with a view to ascertain whether the motion of either the mercury or spirit has been reversed in an unusual manner; and such double maxima or minima, when remarkable, should be recorded as 'supernumerary,' with their dates and leading features.*

"Self-registering Thermometers are extremely apt to get out of order, by the indexes becoming entangled in the column of fluid. In traveling, they should not for a moment be carried with the mercury bulb downwards; if this should happen, they are *sure* to arrive in a state unfit for use. To correct them is tedious, and always hazards fracture. With great care, however, it may be done, as follows:—

"1*st.* The Spirit Thermometer. By many jerks, force the index down to the junction of the bulb and tube; then, by cautiously heating and cooling alternately the bulb, the tube, or the air-vessel at the top, as the case may require, the disunited parts of the spirit may be *distilled* from place to place, till the whole is collected in one column in union with the spirit in the bulb.

"2*nd.* The Mercurial Thermometer. When the steel index gets immersed in the mercury, it cannot be moved by a magnet, and lets the mercury pass by its side. First cool the bulb (by evaporation of ether, if necessary) till the mercury is either fairly drawn down below the index, or a separation takes place in the column, leaving the index with mercury above it. Endeavour then, by tapping, warming the tube, or by the magnet, to loosen the index ever so little, then apply heat to the bulb, and drive up the index, with its superabundant mercury, quite into the air-vessel. This requires many trials and much patience. When there, hold the instrument bulb downwards, and suspend the index by a magnet at the top, allowing any globule of mercury to drop into the origin of the tube below; then heat the tube cautiously over a very small clear flame of an oil lamp, till the mercury rises to the very top of the tube, and fairly unites with the globule there awaiting it. Let the bulb cool, and the mercury will sink in one united column; if not, heat it again. When this is accomplished, the index may be set loose, by withdrawing the magnet, and restored to its proper position in the tube.

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* "The spirit thermometer is apt to undergo a gradual change of zero by the transfer (by distillation) of part of its spirit to the upper end of the tube. It should, therefore, often be compared with the mercurial one, and the difference of readings *applied* as a zero. In this *only* case is the application of a zero *before* registering permissible, and indeed essential."

"A self-registering thermometer may be advantageously left (properly secured) for a whole year, or parts of a year, on elevated summits or other remarkable points, to ascertain their maxima and minima of temperature during absence. In such cases, take care to defend them from discovery, or accident from animals, birds, snakes, &c. In taking it up for reading off, observe not to derange the indexes, and do not leave it without seeing that the indexes are in contact, and the temperature that of the air at the moment.

"*Of Thermometers buried in the Earth.*—Thermometers buried at different depths, for the purpose of examining the monthly changes of temperature of the soil, should have their balls and lower part of the scale well wrapped up in a woolen cloth or pounded charcoal, and should be placed in strong earthen vessels which may be entirely withdrawn from the ground so as to allow of inspecting and reading off the scale, without exposing the balls to any possibility of changing their temperatures while under examination. The vessels should be fitted with covers, to defend the scale from injury in burying and digging up.

"A pipe of earthenware (composed of separate pieces,) or one of wood, may be sunk ten or fifteen feet below the surface, into dry earth, and a thermometer, defended as above, lowered *by a chain.* The pipe being then obstructed at every two feet by some stuffing readily hooked up, the thermometer may be easily examined, and a register of its indications kept with very little trouble. In like manner, the temperature of wells may be registered."

[TO BE CONTINUED.]

A comparison of the late Imperial Standard Troy Pound Weight with a Platina copy of the same, and with other Standards of authority." *Communicated by Professor* SCHUMACHER, *in a Letter to* FRANCIS BAILEY, Esq. V. P. *and Treas. of the Royal Society.*

Professor Schumacher being desirous of procuring an accurate copy of the English Imperial Standard Troy pound weight, for the purpose of comparison with the Danish weights, applied to Capt. Kater, requesting him to cause such copy to be made; which was accordingly done. It was made of brass by Bate; but the result of the weighings not being satisfactory to

Professor Schumacher, he desired to have a second copy forwarded to him. As these two copies did not agree in their results, the first was returned to Capt. Kater with the request that he would repeat the weighings. The result confirmed Professor Schumacher's suspicions: and as it was not thought proper that, in an affair of so much importance as the comparison of the standard weights of two nations any source of discordance should exist, or even be suspected, (the preceding experiments having been made with a *copy* of the Imperial standard weight) the Danish Government sent over Capt. Nehus (of the Royal Danish Engineers) to this country for the express purpose of making comparisons with the *original* standard, in the possession of the Clerk of the House of Commons.

The weighings took place in the Apartments of this Society, and were partly made with Ramsden's balance, belonging to the Society. Besides the first brass weight above mentioned, there was another brass weight made by Robinson, a platina weight made by Cary, the brass pound weight belonging to the Royal Mint, and the platina pound weight belonging to this Society. These were all subjected to a most rigid and accurate series of weighings by Capt. Nehus, in which every precaution was taken to insure the most correct results. It would be impossible here to follow Capt. Nehus through all his details: but it may be sufficient now to state that upwards of 600 comparisons were made with the English Imperial standard, all of which are apparently very accordant; but, on account of a singular circumstance connected with the *original* standard, do not possess that degree of precision, nor afford that satisfaction which ought to attach to an affair of so much importance. For, it appears that not only the specific gravity of the original standard had never been ascertained, but that we are even ignorant of the kind of metal of which it was composed: some persons maintaining that it was of brass, others of copper, and others of bell-metal. And as the original was totally destroyed in the late fire which consumed the two Houses of Parliament, we cannot now supply this omission. It is well known that the specific gravity of brass may vary from 7·5 to 8·5; so that a difference of at least $\frac{1}{8}$ of a grain might arise from this circumstance alone; setting aside a number of other particulars that require minute attention, and which do not seem to have been attended to in former experiments of this kind. In fact, as Professor Schumacher remarks, though we have thus five different pounds in excellent preservation, and compared with the lost standard, with the greatest care and the best instruments, though the number of these comparisons exceeds 600, yet there still remains, an uncertainty as to its real weight; and this solely on account of specific gravity and expansion not being known. And, he adds, that it is to be hoped that no pound will in future be declared a legal standard unless these elements (the knowledge of which is indispensable even for a single comparison with a good balance) are previously determined with the greatest possible precision.

Besides the account of these numerous weighings, which are stated in detail, Professor Schumacher has given various formulae and tables which will be found of great use and application in any future experiments of a like kind that may be undertaken. Proceed. Royal Soc.—Lond. & Ed. Philos. Mag.

Fossil Infusoria.

One of the most remarkable discoveries of modern times is the fact ascertained by professor *C. G. Ehrenberg*, and communicated by him in several successive memoirs to the Royal Berlin Academy of Sciences, that a number of siliceous rocks and minerals, and especially the hard substance used in polishing, called Tripoli, is composed almost entirely of the remains of infusory animals.

"We may regard (he states) as fully ascertained facts, that the following substances consist, either entirely or in part, of the shells of loricated infusoria, viz.

1. *Bergmehl*,	Of the newest formation.
2. *Kieselguhr*,	
3. Polishing slate.	Tertiary substances.
4. *Saugschiefer*,	
5. The semi-opals of the polishing slate,	

We may consider the following minerals as *very probably* of the same description :—

6. The semi-opal occurring in Dolomite,	Secondary and primary formations.
7. The precious opal occurring in porphyry,	
8. The flints occurring in chalk,	
9. Yellow earth,	Of the newest formation.
10. Bog iron-ore,	
11. Certain kinds of lithomarge.*	

In a subsequent paper the author remarks:

Several years ago, the author discovered that the ochre-yellow slimy substance, which sometimes covers the soil in marshy rivulets and ditches, and seems often to be regarded as a deposition of oxide of iron, is a very delicate form of the Bacillarie, which, when heated, becomes red, like the oxide of iron, and is very ferruginous; but which neither loses its form by ignition, nor by the action of acids, and therefore must possess a siliceous covering, that approaches most nearly the genus *Gaillonella*. Hence, this form was, during the previous year, figured as the *Gaillonella ferruginea* in plate 10 of the work on Infusoria, which is about to appear. All the ochre surrounding bog iron-ore exhibits siliceous filiform members, as the residue after the removal of iron. The above appearances render it extremely probable that the Gaillonella ferruginea performs a very important part in the formation of bog iron-ore, whether it be by the direct addition of the iron contained in itself, or by attracting that belonging to neighbouring foreign substances.

The slaty Tripoli of commerce also consists almost entirely of infusory animals. The polishing slate of Bilin, in Bohemia, which forms whole strata, consists almost entirely of a minute infusory animal, which can be referred to the genus *Gaillonella* (*Gaillonella distans*.) The *Podosphenia nana*, n. sp., *Navicula scalprum?* and *Bacillaria vulgaris* (the two last are recent marine species) occur singly between the individuals of the prevailing species, and the first only can sometimes be compared to the Gaillonella for abundance. In the same polishing slate there occur impressions of plants and one species of fish, the *Leuciscus papyraceus* of Broon, according to Agassiz. In the adhesive slate of Menilmontant, some indistinct traces were found of the *Gaillonella distans.* An individual of this animal which, almost without any uniting basis, constitutes the polishing slate of Belin, is $\frac{1}{288}$ of a line in size; many are smaller; there are consequently forty-one thousand millions of these animals in one cube inch of that substance. Ed. New Philos. Jour.

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* The examination of a boulder found in the *Mark Brandenburg*, which had been regarded as a floatstone or *Schwimmstein* (Quarz-agathe nectique, H.,) has recently proved to me, that its principal mass consists of unattached siliceous spicula of sponges, and of the small globular bodies (infusoria, Pyxidiculis ?) which the flint pebbles of the district contain in great abundance. These bodies occur in the powder of the flinty envelope. The *Schwimmstein*, therefore, bears the same relation to the flint as the polishing slate to the semi-opal, and is to be referred to the chalk.

Progress of Civil Engineering.

On the Expansion of different kinds of Stone from an increase of Temperature, with a description of the Pyrometer used in making Experiments. By ALEXANDER J. ADIE, Civil Engineer.

(Read 20th April, 1835.)

For a long time I was anxious to know the rate of expansion of the common building stone of this neighbourhood, as it is not given in any of the tables of the expansions of substances, because I have sometimes thought that the vertical cracks frequently seen intersecting rubble walls might arise from the contraction caused by a diminution of temperature. During a long continued and severe frost which occurred in 1826, I thought the rents in a considerable stretch of wall, which I passed at all seasons, appeared more open than usual. This, however, was merely conjectural, and I paid no more attention to the subject until 1830, when, as I mentioned in a notice on the commencement of my experiments read at the meeting of the British Association in September last, an interdict of the Dean of Guild Court of Edinburgh, rendered the rate of expansion of stone a matter of more importance than merely a curious philosophical speculation. The interdict was issued against a gentleman who wished to alter his property, by supporting the front on cast-iron pillars, in the manner more commonly followed in this city in converting the ground-floor of dwelling-houses into shops. The front of the premises alluded to was narrow, and of a great height; and the reasons given for interdicting the operations were, that the pillars had not sufficient strength to support the weight of the front, and that the difference of the expansion of cast-iron and stone was so great, that very prejudicial effects might arise from the use of such pillars in this situation: Mr. JARDINE, civil engineer, was desired to report to the Dean of Guild on the subject, and the proprietor requested my father to calculate the comparative expansibility of the cast-iron pillars and the stone, for a considerable alteration of temperature. Sufficient data were easily got for the rate of expansion of cast-iron; but the only experiments which could then be found on that of stone, are contained in a short notice by M. DESTIGNY, in the 7th volume of the *Quarterly Journal of Science, Literature, and Art.* These are not extensive, and have, moreover, been determined by taking the difference between the expansion of the stones and rods of iron and copper to which they were attached, thereby diminishing quantities naturally very small; and the difference was again augmented and rendered more visible by a double system of levers: a method liable to many objections in point of accuracy, even though the experiments were performed with great care.

Since the date of the interdict formerly referred to, the almost universal adoption of cast-iron supports in constructing elegant fronts for shops, and the perfect manner in which they have been found to answer the purpose, has given that practical refutation to the alleged objections which, in such cases, is always most to be relied on. And although I do not recollect that in any instance, a fire has taken place in a building, the front of which was supported on cast-iron, yet, from the experiments I have made, it will be seen that even under such circumstances the iron is not likely to be hurtful from the excess of its expansion over that of stone. Nevertheless it appears to have been a common opinion until within a very recent period, and one which has been supported by names of the very highest authority, that the length of a rod of Carrara marble could not be sensibly altered by a change of temperature. This opinion was founded on a very beautiful speculation on the arrangement of the crystals of marble; and although none of my experiments tend to show that it is correct with regard to either of the specimens of Carrara marble which I have examined, yet the supposition may not be altogether groundless, as I have found no stone which expands less than the black marble from Galway in Ireland.

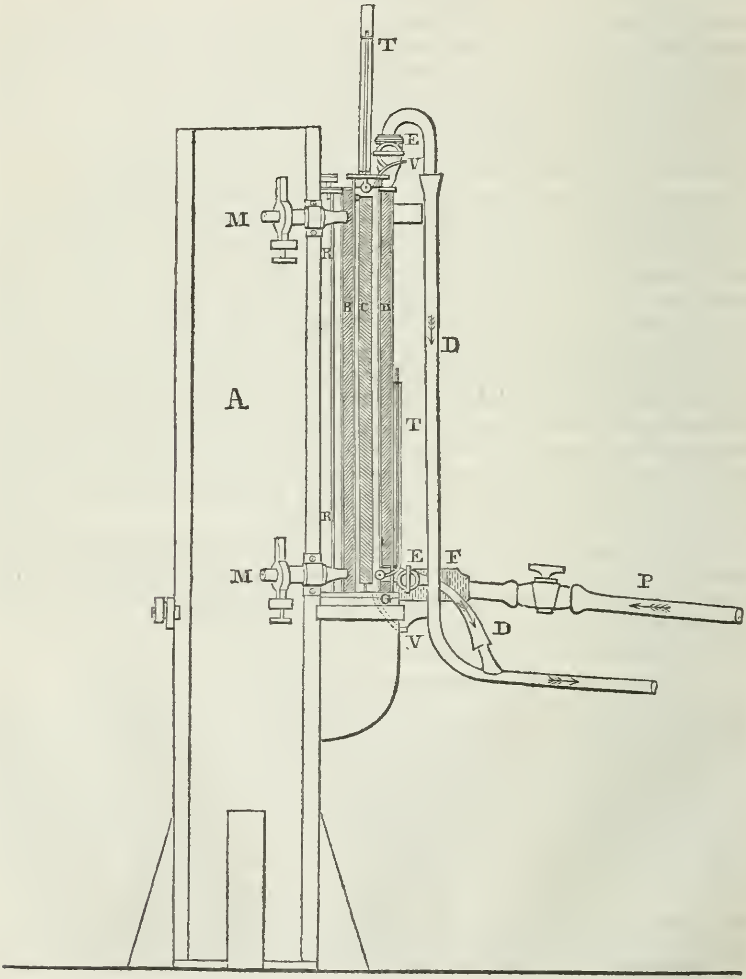
Since the commencement of my experiments, I have seen a notice in the *London and Edinburgh Journal of Science*, of a letter read on the 12th of March, 1834, to the Geological Society of London, by Mr. Charles Babbage, in which the author states, that, from the experiments of Colonel Totten, recorded in Silliman's Journal, he has calculated a table of the expansion, in feet and decimal parts, of granite, marble, and sandstone, from which he finds the alteration in bulk so great, that, supposing the strata under the temple of Serapis to expand at the same rate as sandstone, and an increase of temperature equal to 100° to act on them to the depth of five miles, the temple would be raised 25 feet. He therefore concludes, that the different changes of level which this edifice has undergone, in reference to the sea, may be accounted for, simply by supposing the temperature of the subjacent rocks to have been altered. The sandstone which Colonel Totten had experimented on has been even more sensible to change than any of my specimens; because I find that a similar increase of temperature acting on a mass of what the marble cutters call Sicilian white marble, five miles thick, would produce an elevation of only 19.8 feet, or about $\frac{4}{5}$ths of the other; and it expands more than any other stone I have tried.

Before making any further remarks on the experiments themselves, I shall give an account of the construction of my pyrometer, and of the manner in which the experiments were conducted, so that every one may be enabled to judge how far he may rely on the accuracy of the results at which I have arrived.

Description of a Pyrometer heated by a current of steam.

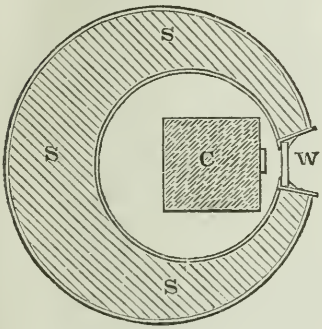
The pyrometer I employed was constructed of a large piece of an oak tree of very straight growth, which was squared to eight inches and a half. In order to get the wood as well seasoned as possible, I selected it from the best of the oak beams got in the old houses lately pulled down in forwarding the improvements of Edinburgh; and from the number of coats of size paint which covered three sides of it, there could be no doubt that it had been long in an open situation. After it was worked to the proper shape, it was allowed to stand for some weeks, as it is sometimes said that, to remove the surface from timber, will cause it to warp, however well it may have been seasoned beforehand. The stand for this instrument, however, did not alter; and even smaller pieces of the same tree, and of another tree from the same place, which were accurately fitted into other parts connected with the experiments, have not altered in the least degree; I therefore concluded that the wood had been very long kept in a dry situation. To the oak stand A, which was 4½ feet long all the other parts of the instrument were attached. B is a double metal case containing the specimen C under experiment. The bottom of the case B rests in a strong brass plate, which is fixed on the top of a block of oak eight inches broad and eleven long, and the block is strongly hooped and bolted with iron to the stand. The upper part of the case B is fixed to the stand by a brass clamp. Fig. 2 is a cross section of the double case of the full size, and the part marked *s s* is the space through which the current of steam passes; so that the specimen C placed within the inner case, can be heated by the steam and yet kept

Fig. 1.



quite dry. P, Fig. 1, is the pipe by which the steam is brought from the boiler and thrown into the bottom of the double case, and D D are eduction pipes, from the top and bottom of the case B, by which the steam is carried off and thrown into the chimney, to prevent the atmosphere of the room from being rendered damp from its escaping into it. On each of the eduction pipes D D there is a stop-cock E, to regulate the quantity of steam which ought to pass off from the top and bottom of the double case, in order to keep the thermometers T T, which are inserted into the top and bottom of the inner case, at the same height; and by regulating the openings of these stop-cocks, the thermometers may easily be kept at the same temperature for any length of time. M M are the micrometer microscopes, strongly fixed to the stand A, and opposite to plate-glass windows W, Fig. 2, which are placed in the double case B, so as to suit the length of the rods of which it was wished to measure the expansion. The upper micrometer reads the thirty-thousandth part of an inch, and the under one was intended to have been used to read a much smaller quantity; but it required great care to draw the lines on the silver studs on which the lengths were laid off, so as to make the upper micrometer read to the same division; it was therefore found more convenient to employ the under micrometer as a fixed one, by moving the zero line of the under stud on the rod till it intersected the angle of the cross spider-threads in the under micrometer. This was done by the screw G working on the long end of a lever, which raised and lowered the rod under experiment; the lever, besides reducing the quickness of the screw G, gave it a more convenient position, and made the rod move without any tendency to turn. The whole body of the double case B was covered with a jacket about the third of an inch thick, made of several plies of flannel; and all the metal parts of the top and bottom, as well as the upper stop-cock E, and the greater part of the upper thermometer, were also thickly covered with flannel. This was found necessary to get the upper and under thermometer to stand at the same temperature. There was a screen R of polished tin-plate interposed between the stand supporting the micrometer microscopes and the double case B, in order to prevent any heat from being radiated from the one to the other. Also, in order to keep the stand of the pyrometer at the same temperature during the experiment, it was placed near the window of the room, and the furnace for the boiler was built in a common fire-place, the front of which was closed with a screen of thick boards; and the polished tin-pipes which conducted the steam from the boiler to the instrument and back again to the chimney, passed through holes in the screen-boards. A detached thermometer was hung over the instrument, and it was always kept at the same height by admitting more or less air by the window. In order to have the same direction of light during each experiment, I employed two small lamps, placed about a foot from the instrument, and their light was thrown in at the windows by lenses and reflectors. The reflectors were fixed on soft wires, so that they could be bent to reflect a bright light from the bot-

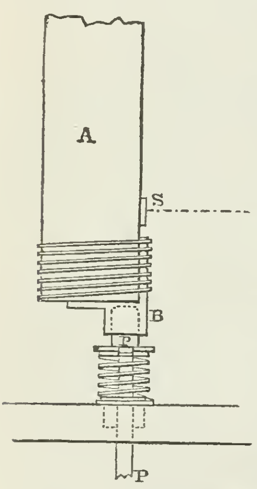
Fig. 2.



tom of the lines on the studs. All the parts of the instrument were made as heavy as consistent with convenience, to prevent their being affected by sudden changes of temperature.

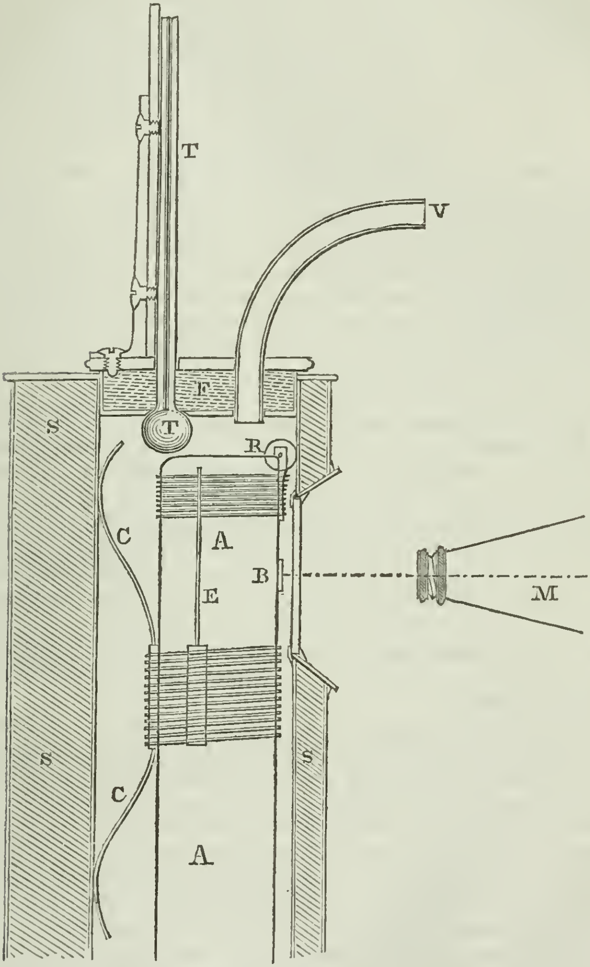
The rods of which the expansions were determined, varied from half an inch to an inch square, and the length of twenty-three inches was carefully laid off on silver studs, which were fixed into holes drilled in the rods, the centres of the holes being twenty-three inches apart. It was necessary to divide the head of the upper stud into fiftieths of an inch, in order to determine the value of the micrometer for each experiment, as it was liable to a little variation, from the impossibility of adjusting it to exactly the same focus which it had when it was adjusted to a fixed value. To guard against this error, arising from alteration of the focal distance, I read the micrometer for $\frac{3}{50}$ of an inch on the stud in the rod which was to be heated; and this was repeated at each experiment made on the rod, after every thing had been adjusted. In placing the rod in the instrument, a socket was tied to the lower end below the lower stud, and the socket was fitted so as to slip tightly upon the top of a square head, on the end of a pin P, Fig. 3, moved by the lever and screw G, Fig. 1. This served to raise and depress the rod, and was very convenient in fixing it in the proper place, for the under microscope. In Fig. 3, A is the lower end of the rod, S the under stud in it, B the socket, which is tied to A, by the piece on the front, so that it was of no consequence what the thickness of the rod might be as the stud was always in the same position in the instrument; *p* is the square pin fitting into the socket, and moved by the screw G, Fig. 1. The upper part of the rod was kept in the proper position, by means of slender springs of wire, which pressed a small roller against the front of the case, so that the upper stud always stood at the same distance from the object glass of the upper micrometer. This is shown by Fig. 4, which is a section of the upper part of the double case of the full size. A is the upper end of the rod under experiment. R the roller pressing on the front of the double case. S S the space through which the steam passes. B the upper stud opposite to the window and upper micrometer M; C C the back spring; E one of the side springs; F the cover of the top which is double; and the space between the upper and under plate is stuffed with cotton. T the bulb of the upper thermometer; V is the upper pipe, by which any vapour coming from the enclosed rod may escape, when it is wished to allow the rod to become drier; but the whole of the inner case is to be so tight as to prevent any evaporation, unless the upper pipe V, and the under one of the same size, V, Fig. 1, which passes through the bottom plate, are left open. The springs and roller represented in this figure were firmly tied on the rods, and could easily be shifted from the one to the other. It was my intention to have ascertained the expansion of the different rods between the freezing and the boiling points by first filling the space, S S, Fig. 4, with melting ice, and then by passing a current of steam to get the higher point, but the difficulty of getting ice until a few weeks since made me abandon

Fig. 3.



this, besides, I found that to keep the thermometers above 208° Fahr., sent a great heat through the room, on account of the strong fire it was necessary to keep up. As the thermometers were made with every precaution to insure their accuracy, I contented myself with the range I could get between the temperature of the room, which was generally under 50°, and 207° or 208°.

Fig. 4.



The rod was put into the pyrometer, the micrometers set, and allowed to stand over the night, and the lowest point taken at the height at which the thermometers stood next day, by making the line on the under stud intersect the angle of the spider-threads of the lower micrometer; and the index error of the line on the upper stud was read from the zero of the upper micrometer. This was repeated four times, by deranging and again adjusting the height of the under line by means of the screw G.

After completing the readings, the steam was turned into the case, and the expansion of the rod measured by a similar set of readings, as soon as the line on the upper stud was found to be stationary, which sometimes required the temperature of the pyrometer to be kept at the same degree for an hour and a half.

I thought that my application of steam for heating a pyrometer was the first that had been done, as I could find no account of its having been formerly used in any of the descriptions of the instruments for similar operations; but Lieutenant MURPHY informed me, that he thinks it was employed in some pyrometric operations connected with the trigonometrical survey in India; and I am satisfied, from the uniformity of temperature it maintains in the instrument, no better method of heating can be resorted to for such operations, because the substance if of wood or stone is kept dry, and the fire or lamps are placed at such a distance as not to incommode the operator in the least degree. Some of the rods of stone experimented on are now before the Society. I had considerable difficulty in making rods of Roman cement, as they repeatedly cracked in drying, and I was anxious to have this substance tried, as I found that rods of lime were too weak, not more to settle the general question, as to the expansion of buildings, than to have the pleasure, if possible, of assisting Mr. BRUNEL in accounting for the results of some experiments he mentioned at the last meeting of the British Association, when describing one of the many beautiful contrivances for which his fertile ingenuity has rendered him so celebrated. I succeeded in making a rod of Roman cement, by at first mixing as little water with the powder as would make it work, and when it had become too plastic, from continued working, more of the powder was added, and the cement again made plastic by working and beating. This process was continued so long as any of the cement in powder could be added to the mass, and the whole again worked into the consistency of soft putty without any addition of water, as the strength of the mortar appears to depend on the smallness of the quantity of water with which we can make the dry powder plastic, this is accomplished by long continued working. After preparing the mortar, it was put into a mould of Bristol board, and when it had set, it was immersed in water, and left for a fortnight to harden.

Having ascertained that all the parts of the pyrometer worked well from the uniformity of the results I obtained in determining the expansion of cast zinc, which was selected as the metal most sensible to a change of temperature, I commenced the experiments for which the instrument was made; but before giving the results of those on stone, I may mention what took place in trying the expansion of a rod of oak, cut from the same tree of which the pyrometer stand was made. On account of a very small quantity of steam escaping through a hole in the inner case, the wood expanded very much the first time it was heated, and when taken out, and allowed to stand some days to dry, I found it lengthened about the thirtieth part of an inch. The inner case was made perfectly air-tight, even under considerable pressure, as it appeared absolutely necessary to prevent steam from escaping into it, for all experiments to ascertain the effect of heat on different kinds of wood; and I also wished to keep the rods of stone quite dry, to be certain what effect moisture had on their expansibility. When the rod of oak was again thoroughly dried, and the length of 23 inches laid off anew on the studs, it was found that for an increase of temperature of 180° Fahr. it only expanded .001426177 of an inch, or .000062007 in decimals of the length of the rod; which is just one-fifteenth part of the amount of the expansion of platinum, the least expansible of the metals. This insensibility to change of temperature in this wood, provided it be kept free from moisture, has induced me to investigate the subject a little farther. I have procured rods of many of the most straight-grained kinds of wood, and after I have determined their rates of expansion, I intend to varnish or cover them with different substances, to find their expansion again, and then to see to what extent, and for how long, they will resist moisture as it has been said that a wooden rod answers best for a pendulum when unvarnished, and left in its natural state.

Table of the Expansion of Stone, &c.

	Decimal of an inch on 23 inches for 180° F.	Decimal of length for 180° F.	Decimal of length for 1° F.	
1. Roman cement,	.0330043	.0014349	.0000075	When the rod contained more moisture it expanded more.
2. Sicilian white marble,	.0325392	.0014147	.0000078	From 1st experiment when moist.
	.0253946	.00110411	.00000613	Mean of three experiments when dry.
3. Carrara Marble,	.0274344	.0011928	.00000662	From 1st experiment, when moist.
	.0150405	.0006539	.00000363	Mean of two experiments, when dry.
4. Sandstone from the liver rock of Craigleith Quarry,	.0270093	.0011743	.00000652	Mean of four do.
5. Cast iron from a rod cut from a bar cast two inches sq.	.0263755	.00114676	.00000637	Mean of two do.
6. Cast iron from a rod cast half inch square.	.0253498	.001102166	.00000612	Mean of two do.
7. Slate from Penrhyn Quarry, Wales	.0238659	.0010376	.00000576	Mean of three do.
8. Peterhead red granite,	.0220416	.0009583	.00000532	From 1st experiment when moist.
	.0206266	.0008968	.00000498	Mean of two do. when dry.
9. Arbroath pavement.	.0206652	.0008985	.00000499	Mean of four do.
10. Caithness pavement,	.0205788	.0008947	.00000497	Mean of three do.
11. Greenstone from Ratho,	.0186043	.0008089	.00000449	Mean of three do.
12. Aberdeen gray granite,	.01815695	.00078943	.00000438	Mean of two do.
13. Best stock brick,	.0126542	.0005502	.00000306	Mean of two do.
14. Fire brick,	.0113334	.0004928	.00000274	Mean of two do.
15. Stalk of a Dutch tobacco pipe,	.0105177	.0004573	.00000254	Mean of three do.
16. Round rod of Wedgewood ware (11 inches long,)		.00045294	.00000251	Mean of two do.
17. Black marble from Galway, Ireland,	.0102394	.00044519	.00000247	Mean of three do.

The preceding Table gives the expansions of the different kinds of stone I have examined, and also of two rods of cast-iron. The one which expanded least, was cast half an inch square, and was put into the pyrometer with the outer surface rough as it came from the mould. The other rod was also half an inch square, but was cut out of the centre of a bar of the same iron cast two inches square, in order to get more nearly the rate of expansion of a large mass.

In the Table, the substances are arranged in the order of their rates of expansion, that which expands most being placed first.

From the results given in the Table, it is evident that no danger is to be apprehended from a change of temperature affecting cast-iron and sand-stone, in a very different degree, as their expansion, in so far as regards all building operations at least may be considered as the same.

The difference between the expansion of bricks and malleable iron amounts only to .00042 for 98° Fah., or half an inch on 100 feet; and Roman cement, when in as damp a state as all building must be, will expand more than malleable iron, since it did so in my experiment, even after it had been dried for eleven hours at a temperature of 207°. This, therefore, will in a great measure serve for an explanation of the fact mentioned by Mr. Brunel, in describing his method of constructing arches, by suspending the courses of brick with straps of hoop-iron, viz., that he had some anxiety as to the manner in which variations of temperature would affect his mode of operating, by expanding the metal more than the brick and mortar; but, on examining his experimental arch, both in summer and in winter, not a crack was to be seen. The substances at the bottom of the Table are black marble, and the rod of Wedgewood's ware; their expansions are nearly alike, and they expand very nearly half as much as platinum for the same number of degrees.*

M. Destigny mentions in his paper, before alluded to, that he found the expansion of stone not at all affected by a difference in its state of humidity. From what I have observed in conducting my experiments, I am quite of a different opinion, for although no increased expansibility was observed by wetting sand-stone after it had been dried, and I did not try any direct experiment of this nature with any of the marbles, on account of the very small quantity of water which they were found to absorb after having been dried, nevertheless the experiments on greenstone show the effect of moisture, and some of the marbles certainly became less sensible to the change of temperature, after they had been heated, and this was probably caused by their natural moisture being driven off. This diminution is distinctly shown by the following series of experiments on Sicilian marble:

1st,	.0325392	Diff. .0058229	Decimals of an inch for 23 inches, the change of temperature being 180° F.
2d,	.0267163	.0013143	
3d,	.0254020	.0013364	
4th,	.0240656		

The rod gave off a considerable quantity of moisture. This gradual diminution of the expansion was scarcely observable in the sand-stones and Aberdeen gray granite, which contained a great deal of quartz; and in the bricks, in the stalk of a Dutch tobacco-pipe, and in a rod of Wedgewood's ware, no such change took place, although all these substances had been wet in cutting the holes, &c. before they could be put into the pyrometer. The Peterhead granite, again, which contains more feldspar than that from Aberdeen, and also the green-stone, were affected by losing their moisture, but more especially the latter, as shown by the following series of experiments made in November. Unluckily the first observation, which always differs most from the others, was lost.

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* I have since procured another rod of Galway black marble, which contained more fossils, and was softer than the first. Its expansion for 180° Fahr. was .0004793, and for 1° Fahr. .00000266.

2d,	.0198379	Diff.	.0017485	Dicimals of an inch, the expansion of 23 inches for 180° Fahr.
3d,	.0180884		.0002037	
4th,	.0178857			

As greenstone absorbs moisture much more readily than the marbles, I replaced the rod again in the pyrometer, after it had hung in the room for more than three months, from the time of performing the first experiments. The instrument was heated to 207° Fahr., and this temperature was kept up without change during the four hours in which I watched the alterations of the length of the rod. The inner case being closed to prevent the evaporation of the moisture, the ultimate expansion of the green-stone for 155° F. was 5.02 revolutions of the screw of the upper micrometer. The pipes V V, for drying the inner case were then opened, a good deal of vapour came off, and when the rod had been allowed to dry for three-quarters of an hour, the micrometer read only 3.24; hence by the effect of drying alone, the rod contracted rather more than $\frac{1}{170}$th part of an inch; I then poured boiling water on the rod, and closed the drying pipes, and although this could not moisten it thoroughly, yet when its expansion was again at the greatest, the upper micrometer read 4.15. The drying pipes were then opened a second time, and although the temperature always remained unaltered, the reading of the upper micrometer was again reduced to 3.32. This experiment clearly shows, that the expansibility of some kinds of stone very much depends on their state of humidity. An analogous experiment was tried with a rod of satin wood, and the length of the wood altered with the similar changes in the same manner, but to a greater extent. With the latter substance this might have been expected, but such an action of moisture on stone is certainly very remarkable. I have therefore given two rates of expansion in the table for several of the rods, the greater is when the rod contained most moisture, the lesser is a mean of several experiments made when it was in a dry state, and when the results of the experiments agreed very nearly among themselves.

A very curious effect of the heat on the white marbles, was that of causing a permanent increase of their length, which, in the Sicilian marble was gradually augmented by the heating for each experiment. The rod of this marble was heated five times, but, unfortunately, as the change is always decreasing, I at first adjusted the micrometers without noting the quantity, by which the line on the stud had not returned to the adjustment for the previous experiment. This was the first time I had met with this gradual increase in the length of the rod; and by the effect of the two heatings noted after I observed it, the rod was lengthened .0096, or nearly the hundredth of an inch, on the rod of Carrara marble, the elongation took place mostly at the first heating, and it amounted .0135 of an inch. This may probably serve to account for the warping which sometimes takes place on those parts of chimney-pieces which are subjected to the greatest action of the fire. It is impossible to say how far this lengthening would be increased by a greater change of temperature; but, since Mr. BABBAGE has applied the expansion of stone to the explanation of geological phenomena, this property of limestone may also be taken into account, as it only requires the heat to have been once in action after the strata have been formed. In the experiment on the rod of Carrara marble, the whole effect was produced by an increase of temperature of 157° F.; and if we suppose this to have acted on strata five miles deep, the depth taken by Mr. BABBAGE, in accounting for the changes of the level of the Temple of Serapis, the surface would have been raised by not less than 15½ feet. Little weight, however, can be attached to such explanations of the great operations of nature, as there may be a vast difference between the effect of heat quickly communicated to the small pieces of stone on which we perform our experiments, and the necessarily slow diffusion of the heat through such a depth of strata as we have supposed.

In order to ascertain if there was any connexion between the density of stones of the same composition and their expansibility, as takes place with the metals, I determined the specific gravity of several of the rods, but the one property does not appear to have the least dependence on the other. For instance, the specific gravity of Sicilian white marble and Galway black marble is 2.7127 and 2.7093, almost exactly alike; the expansion for 180° F. of the former is .0325392, while that of the latter is only .0102394, not one-third of the first. In determining the specific gravities, the stones were first dried on a sand bath, and, after being weighed in a dry state, they were put into distilled water; and although the pieces of the black and white marble weighed about 200 grains, and were left three weeks in water, they only absorbed one-tenth of a grain, yet the white marble, in particular, gave off a considerable quantity of moisture when first heated. The Arbroath pavement absorbed $\frac{1}{28}$ of its bulk of water, and the Caithness pavement $\frac{1}{247}$ part; which shows the great superiority of the latter pavement for all purposes, where it is wished to exclude dampness.

In conclusion, it may be observed that, from the results of the experiments given in the Table, it is perfectly evident that not the slightest danger can arise from the use of cast-iron in buildings, on account of the difference of their expansion for all ordinary temperatures. Nor do I think that there is any cause to dread the effects of fire, where such pillars are used, although that was one of the arguments employed to retard their introduction; on the contrary, there is every reason to believe, that even in cases where the fire would rage with all the fury that a strong wind could impart to it, cast-iron pillars would support the lintels of the windows as safely as those of stone. And I do not state this as a mere matter of speculation. The late great burning of the North Bridge New Buildings, is a proof of the correctness of part of the opinion. Of the extraordinary fierceness of this conflagration, those who did not witness it might easily have satisfied themselves, by inspecting the injury done to the stones of the ashler front. I was present at this terrific scene of destruction, before the flames burst from the windows, and remained till the fire was quite under the power of the engines. Very soon after the flames came sweeping out at the front window, the stones began to crack and skirt off, and a constant shower of such large masses fell, during probably half an hour, that the firemen were obliged to draw back their leathern hose to a considerable distance from the buildings; and, so long as the stones continued to fall, it was impossible to save any of the very valuable property from the shops below, which, but for this might have been easily and leisurely done. Now, no part of the stone was at any time red hot, and the firemen were standing on the window-soles very soon after the flames retired from them, so that no danger was to be apprehended from the risk of cast-iron being melted in such a situation. Indeed, the cast-iron columns in the interior of the building did not appear to be much injured by the fire.

Edinburgh, April 4, 1835. Trans. Royal Soc. of Edin.

First Russian Railroad, from St. Petersburg to Zarscoe-Selo and Pawlowsk, established by Imperial Decree of 21*st March*, 1836, *and carried into execution by a Company of Shareholders in Russia, England, and Germany.* Translated from the German. St. Petersburg, 1837.

The following brief analysis, and extracts (taken from the London Mining Journal and commercial Gazette) of the first attempts at Rail Road making in the Russian Empire, cannot fail to interest the readers of this Journal. The extraordinary skill and energy of the Chevalier von Gerstner, to whom the execution of the whole scheme was confided, and the no less extraordinary rapidity and success with which the work appears to have been accomplished; must challenge the admiration of all who feel an interest (and who does not) in improvements of this nature wherever they may be effected. G.

In our former notice of this pamphlet we gave a brief history of the origin of the railway now forming, and indeed nearly completed, from St. Petersburg to Zarscoe-Selo and Pawlowsk, being the first work of the kind in the Russian empire, and probably only the precursor of others of far greater extent and commercial importance. We pointed out the extraordinary privileges with which the company was endowed, and the spirited mode in which the undertaking was commenced and carried on, under difficulties of some magnitude as regards the supply of the necessary materials and machinery.

We now return to the work, chiefly for the sake of making such extracts as are likely to prove interesting to our readers, as showing the spirit and activity which prevails in that vast empire, which, both politically and commercially, must be an object of no inconsiderable interest to all the other nations of Europe, and of whose advances in the development of her mineral wealth, we lately furnished a notice of considerable length. We shall first quote the introductory pages, omitting merely some tabular statements, as being of secondary importance:—

"The reports on the Zarscoe-Selo Railroad, which from time to time have appeared in the Russian papers, and from thence have been copied into the foreign journals, have excited considerable interest in the public, particularly in Germany and England. No undertaking of the same class has hitherto made such rapid progress as this railway, which, called into existence by the especial patronage of the Emperor of Russia, and endowed with most extensive privileges, precludes all doubt of its proving eminently successful and advantageous to the shareholders. In a few days subsequent to the Imperial grant being obtained, the company of shareholders was formed, and the whole capital (three millions of rubles) subscribed. The payments of the calls upon all the shares issued, were made with punctuality, and no single instalment remained in arrear. Within six month, and six days from the time the decree received the imperial sign-manual, the requisite supply of rails and materials, carriages and machinery, was obtained from England; most of the works on the entire length of lines twenty-five and a half wersts, were completed, the rails partly laid down, and three wersts opened by horse power. Six weeks later the opening of a distance of seven and a half wersts, with locomotives, took place; and this summer (1837) the whole of the line, from the centre of the capital to the terminus in Pawlowsk, will be opened.

"This rapid progress of the undertaking, which in other countries would have been the work of several years, naturally excited the attention both of natives and foreigners.

"That portion of the English public which takes a general interest in railways, expressed a desire to obtain the reports which have hitherto appeared, that they might be enabled to investigate the circumstances that led to so extraordinary a result in Russia; another portion, better acquainted with the favourable state of the Russian share market, with a view to partake in this new speculation:—but as the publications of the Chevalier von Gerstner—viz:—

"*Memoir on the advantages of a Railroad from St. Petersburg to Zarscoe Selo and Pawlowsk, 20th March*, 1836.

"*First report on the progress of this Railroad, 20th July*, 1836.

"*Second report on the same, 22nd September*, 1836; have for some time been out of print, the

"*Third report, 29th January*, 1837, which has reference to the preceding, would hardly be understood by many readers. The purport of the present paper is, therefore, to lay before the public in this country a clear statement of facts relative to the Zarscoe-Selo railway, to assist them in forming a correct opinion of the enterprise, and in entering into it, as well as into other manufacturing concerns about to be carried into effect in Russia. It will tend, at the same time, to give a more expanded view of the internal constitution of that colossal empire, and to correct the erroneous opinions that have been induced by a defective knowledge of its actual condition.'

"PETER the Great, the immortal founder of the power and greatness of the Russian empire, felt how necessary to its welfare was the improvement of its communications; he had witnessed in Holland the beneficial influence of canal traffic; he visited the interior of his empire, and himself planned the whole of the water communications which were, either during his reign or afterwards, carried into execution. This sovereign introduced *Canals* because, in the then existing state of knowledge, they were considered as the most perfect channels of internal communication. ALEXANDER I. introduced artificial *Roads*. He commenced with the first turnpike road from St. Petersburg to Moscow, a distance of about 700 wersts, the smallest portion of which only was accomplished when death overtook this monarch; but his successor, the reigning Emperor NICHOLAS I., carried out the project; in a few years finished the road to Moscow; and caused surveys to be made for a complete system of roads, intended to intersect the whole interior of the empire, which, under his happy reign, is now making such rapid advances in prosperity.

"The progress which *Railroads* had made in modern times did not escape the scrutinizing view of the sovereign. The Chevalier von Gerstner went to Russia in August, 1834, with the intention of visiting the interior of the country, and informing himself respecting its manufactories and mines. The Emperor Nicholas heard of this, as well as that so early as 7th September, 1824, the Chevalier had obtained a privilege from the late Emperor Francis of Austria, to construct a railroad between the Moldau and Danube; that for four years, up to 1828, he conducted the works on this line, comprising, with its continuation from Linz to Gmunden, a total length of 130 English miles, over which the traffic continues in summer and winter without interruption. The Emperor in consequence, in September, 1834, expressed a desire to see a line of railway from St. Petersburg to Moscow executed, if possible, by a company of shareholders.

"After the Chevalier von Gerstner had in the beginning of 1885, finished his tour to the manufacturing provinces, he was presented at the Court of St. Petersburg, when the Emperor, with great earnestness, and with the penetration for which he is so remarkable, expressed himself strongly as to the advantages that would result from the introduction of railways into Russia, and the extraordinary privileges that the first undertakers might expect.

"The Chevalier von Gerstner in consequence proposed to commence with two short lines, the first from the interior of St. Petersburg to the towns of Zarscoe-Selo and Pawlowsk, and the second from the same point in the capital to Peterhoff and Oranienbaum.

"The negociations for the grant lasted to the end of the year 1835, when, on the 21st December, the President of the Council, in the name of the Emperor, communicated to M. von Gerstner that he was thenceforth invested with the *exclusive* personal privilege of *incorporating shareholders for the execution of both railways.*

"The Chevalier von Gerstner hereupon joined the three other directors and founders of the company of shareholders, whose names have been given above, for the execution of the first line, from St. Petersburg to Zarscoe-Selo and Pawlowsk, whilst he reserves to himself the right of forming the company for the line to Peterhoff.

"The demand for shares, on the undertaking of the enterprize being made public, was so great that the first 15,000 shares, or the original capital of three millions rubles, were subscribed for almost immediately; chiefly by the Russians and naturalized Germans, although persons residing abroad may participate, without any restriction. The latter may either appear in person at the general meetings, or be represented by their agents, and receive their dividends the same as the Russian subjects, without any deductions or the payment of any duties to the state.

"The Imperial Grant for the Zarscoe-Selo railway is dated 21st March, 1836. The privileges thereby conceded to the company are very considerable, such as were never granted to any railway company in any country before. The execution of the railway is regarded as if undertaken immediately by the Crown; the Crown lands have been gratuitously ceded to the company; the farmers holding lands that were required, have been appointed to other ground, and are compensated by the Crown for any loss sustained by the transfer; lands or buildings, the property of private individuals, must be surrendered to the company, either by voluntary agreement or at a price to be determined by judicial valuation; but to prevent the obstruction of the works, the company, by depositing a sum of money about equal to the purchase price of a similar plot of ground or tenement in the vicinity, have the power to take possession of such lands or tenements before the termination of the appraisement. The valuation being determined, the balance of the amount due is paid to, or received from, the parties.

"The company are at liberty to erect any description of buildings requisite for the railway traffic, for 100 fathoms on each side of the railway, except in the Artillery Gound, through which the line passes, and for the acquisition of which the same privileges have been granted as for that of the rest of the line. The removal of the battery, rocket manufactory, and other military buildings intersected by the line, is to be effected by the company, at their cost, to another quarter. In this manner the railway without the town forms an uninterrupted straight line for twenty-four wersts. Within the town the straight line is only warped into two gentle curves, by following the course of the Wedenskoi canal.

"The medium rise of the whole line is one in 1028, and the extreme one in 504. The railway terminus in the town is at the junction of the Wedenskoi with the Fontanka canal, on a piece of ground eighty fathoms by forty-two and a half in breadth, which has partly been purchased and partly ceded by the Crown. From this spot to the new boundary of the town on the Ligofka, the railway measures about one and a half mile, and will, therefore, for that distance, *run within the capital,* a circumstance of the utmost importance, as effecting the number of passengers or the amount of traffic. The terminus at the other end is situated 550 fathoms within the Great Park of Pawlowsk the property of his Imperial Highness the Grand Duke Michael Pawlowitsch. The company have been permitted to erect, in some of the finest parts of that park, several buildings for the reception and entertainment of the public; and at Zarsco-Selo they have been allowed to establish an hotel at the railway station.

"The company have the right to purchase the iron for the whole line, abroad, and to import the same *duty free,* provided no Russia iron worker should undertake the delivery in the required quality, form, and time, and at most at fifteen per cent. advance upon the price at which the iron might be imported into St. Petersburg, from foreign countries. The company are further empowered to import the locomotive engines, railway carriages, and all other machinery and requirements, *duty free.*

"The company are not bound to any fixed fares for the conveyance of passengers, or rates for the carriage of goods, but are at liberty to fix them at discretion. The railway remains *for ever* the property of the company: during the first ten years no one can make a railroad in the same direction; and during the same term of ten years the company are exempted from the payment of rates and taxes of every description; either to the Post Administration or any other authority. The capital of the company is covered by 15,000 shares of 200 bank note rubles each; if necessary, 2500 more shares may be issued for raising the reserve fund of 500,000 rubles; all the 17,500 shares, however, participate alike in the profits of the undertaking. *The number of shares can in no case be augmented.*

"Eight days after the grant had been made out, the Chevalier set out from St. Petersburg for England and Belgium, for the purpose of ordering the necessary rails, engines, carriages, and other railway machinery, as no one could be found in Russia to contract for these materials in the stipulated time.

"It was exceedingly difficult to obtain them in England last year, as the iron works there, in consequence of the many English and American orders, were occupied literally night and day, and most of them had employment for a year or two in advance, in consequence of which the price of rail bars had been raised upwards of forty per cent. within a twelvemonth. Another difficulty arose from the Chevalier having altered the *Gage* (or the distance of the two rails upon which the carriages run) of the Russian railroad, from that established in England. On the old English railroads only goods of small bulk and great weight were transported, such as iron, coals, stones, bricks, &c., but not sheep's wool, hay, straw, or fire-wood. In 1822 when the railway between Stockton and Darlington was begun, which was first intended for a general traffic of passengers and goods, Mr. George Stephenson, the engineer, established the breadth of the track between the rail at four feet eight and a half inches English, as being the width of the track of carriage-wheels on high roads. Experience has shown how inconvenient this arrangement is for the locomotive engines, which in England, usually of thirty to thirty-five horses' power, are by this narrow gage confined within about four feet, which is by far too little for such an engine. The driving wheel can at most have but a diameter of five feet, as otherwise it would lurch too much; in order, therefore, to do thirty miles an hour, it must make 168 revolutions in a minute. The strain and wear and tear of all the parts of a locomotive, by reason of the quick motion of the driving wheel, and more particularly the cramped arrangement of the individual parts, are therefore very considerable.

"The disbursements in the coaching and carrying departments on the Liverpool and Manchester railway, upon an average of the last three years, amounted to forty-four per cent., and the repairs of the locomotives to fifty-six per cent. of the total expenditure, originating in their wear and tear as a tractive power. This charge must naturally be far less in a wider gage. If cattle and bulky materials, such as sheeps' wool, straw, hay, fire-wood, travelling carriages, &c., are to be conveyed, the load cannot be stowed between the wheels, if the gage is only four feet eight and a half inches, but must be placed in a box or on a platform six to eight feet wide, by which the base being only half as large as the superstructure, great lurching is necessarily occasioned, particularly at high velocities; and moreover, especially in rough weather, high loaded waggons, with a confined base, are apt to have the flange of the wheels rub up against the rails, thereby occasioning great increase of friction, wear and tear. The trains generally run the distance between Manchester and Liverpool in one and a quarter to one and a half hour, whereas in a high wind those laden with cotton or wool take three hours for the journey, which would not be the case if a wider gage had been adopted. Increase of axle-friction cannot take place on a wider gage, as in all railway carriages the friction is now no longer between the wheels, but on the outside, on the projection of the axle through the wheel.

"These and other reasons induced the Chevalier von Gerstner to *adopt a gage of six feet English between the rails;* but the consequence was, that for the locomotive engines, turnplates, and machinery, new drawings and models had to be prepared, before the construction of the machinery, could be commenced. The deliveries thereof last year were in consequence attended with considerable difficulty; but connexions of many years standing which the Chevalier von Gerstner had in England, enabled him to overcome these difficulties, and in the Third Report there occurs an account of the rails, chairs, &c. imported duty free into Petersburg."

Passing over some details of the work, and the partial opening of the railway, from which it appears that it progressed satisfactorily, and that the snow occasioned no impediment on the line, we come to the following extract from the Chevalier von Gerstner's third report :—

"Whoever will bear in mind that the Imperial Decree was dated on the 21st March, 1836—that until then I could not take my departure for Belgium and England, for the purpose of giving the orders in those countries for the railway materials—that these orders came when the English manufactories and iron works were occupied night and day, to the utmost stretch of their capabilities; whoever will bear in mind that the gage of the English railways is fifty six and a half inches, whilst ours is seventy-two inches, and therefore, that not a locomotive, not a carriage or a turntable could be purchased and imported that had not to be planned and have new models expressly made for it, after which, and not until then, the various machinery could be put in hand and made; whoever will further consider that every rail and piece of machinery, when finished, had to be conveyed to some sea port town, there to be shipped for Cronstadt; on its arrival there, to be transhipped into lighters, to be brought on the Fontanka canal to St. Petersburg, again unloaded, and, frequently on the worst of cross roads and marshes, carried to the point of their destination on the railway, there to be laid down and adjusted or fitted up; whoever will calmly examine and weigh all these matters, will not withhold from the direction of the railway of which I have the honour to be a member, the testimony that nothing but the greatest ardour and spirit, systematic management and punctuality, especially in the payments, could, in so short a time, have brought about a result, which in other countries, it generally takes years to achieve.

"As by the arrangement which the directors concluded with me in the name and on behalf of the company, and by virtue of section 10 of the Imperial Ordonance, the works were committed to my management under certain conditions, involving great responsibility, I lost no time in applying all my energies to the promotion of this great undertaking.

"Although I personally directed the works along the entire line, and traversed it continually on foot from St. Petersburg to Pawlowsk and back again, I nevertheless thought it right to employ seventeen engineers, five of whom had visited the railways in England, as superintendents; besides these, upwards of thirty inspectors, and as many watchmen and soldiers were periodically present on the line. For erecting the machinery and as enginemen for the locomotives, ten Englishmen and two Belgians came over—of the former five have returned.

"Solidity of materials and accuracy of workmanship,—strict integrity in all money transactions;—the greatest publicity in the accounts, —indefatigable attention to the progress of the undertaking,—system and order in the management, were the points I kept in view from the moment in which I commenced this enterprize. By the observance of them I have brought the work, so far as regards my own department, to its present state; by similar conduct I shall bring it to an honourable issue. Russians, Germans, Englishmen, Italians and Belgians, co-operated in our undertaking, and a stranger might not unreasonably have been astonished when, on the point of the line at the building at Pawlowsk he heard five languages spoken and found the people of five different nations here, at so distant a point of Europe, unite for one purpose—the promotion of our great object.

"I consider it my duty to state, and do so with pleasure, that the Russian population, so far from throwing obstacles in our way, have invariably been anxious to assist in the execution of the work.

"Incredible as it may appear to my foreign readers, I can state as a fact that no opposition has been evinced (as has almost invariably been the case in other countries) to so great an innovation. The proof is easily adduced, for not a single complaint of premeditated obstruction has come before the authorities. Our difficulties hitherto have been confined to the negociations with the proprietors of the plot of ground between the Ligofka and Fontanka, being town lands; without the town the farmers freely ceded their lands; on the first intimation being given to them by the surveyors, they cleared their forests to the width of sixty fathoms—nay, they mowed the half ripe corn in their fields, in confident reliance that the compensation the law has awarded them would be duly paid. The peasantry from the whole surrounding country worked on the line; and they were the first, who upon its opening crowded into our railway office, there paid their forty kopeks for a seat and availed themselves of the new conveyance. The coachmen and inkeepers at Zarsco-Selo and Pawlowsk, immediately perceived the prospect of gain which was opened to them; the former arguing the increase of their business in the conveyance of such a number of persons to the stations, and the latter that they will have to accommodate crowds of visiters whom the company's buildings will be insufficient to contain."

Our concluding extract will furnish some account of the object of the Zarscoe-Selo Railway, and the estimated amount of traffic and of profit to the shareholders:—

"St. Petersburg, the modern capital of the Russian empire, was founded by Peter the Great in 1703, at the entrance of the Neva into the Gulf of Finland, in a low, and formerly quite a marshy country. However advantageous this site may be for trade, it has been attended with incalculable disadvantages; amongst which may be reckoned the periodical inundations, of which the 7th November, 1824, afforded so dreadful an example; the changeable state of the weather and temperature, the fogs which envelop the town, the difficulty of constructing sewers for carrying off the impurities from the streets, and of laying down pipes for spring water. The canals which intersect the town are no doubt of importance for the conveyance of fuel and building materials to the town; but they likewise serve as receptacles for filth, and in the heat of summer fill the air with offensive effluvia. The sewers of the principal streets, for want of sufficient fall are at intervals obliged to be opened, and the contents laid in the streets and carted away.

"The consequence is, that the exhalations, especially in warm weather, are very unwholesome, and hence the urgent necessity that the inhabitants of St. Petersburg should, at that season, leave town to reside in the country.

"Hitherto the country houses have been confined to the adjacent islands, elevated only a few feet above the Neva, and in spring and autumn, therefore, covered with fogs. These islands, nevertheless, were generally filled with temporary residents, and country houses continued to spring up in the immediate vicinity of Petersburg. The cheerful town of Zarscoe-Selo is sixteen English miles from St. Petersburg, situated on an eminence bordered by the Neva. The imperial palace there is 220 feet higher than the Neva; the town of Pawlowsk, two and a quarter English miles further, lies 105 above the Neva. The charming situation of these two places, and of the imperial parks adjoining, are sufficiently acknowledged and admired; but owing to the distance from the capital, not above 300 families could annually reside there in summer. Notwithstanding the distance, however, both were much frequented on Sundays.

"For a conveyance to Zarscoe-Selo or Pawlowsk, 18s. to 22s. sterling is paid; for a place in the stage coach three and a half rubles, and as much back; nevertheless from inquiries instituted by government in 1834, the number of equipages passing from St. Petersburg to Zarscoe-Selo and Pawlowsk and back again, for one year, was 70,386, employing 178,187 horses. At the rate of one passenger for each horse, the present annual transit of persons would thus be 178,187, of whom half or 89,093 make the journey there and as many back. This great traffic may be attributed to the delightful situations of the towns, as well as to the circumstance that in Zarscoe-Selo and Pawlowsk five regiments, with 300 officers, are stationed, including the two depot regiments, one of infantry, and one of cavalry, the officers of which change every year or two, as they only learn the routine of duty here, and then return to their stations in the interior of the empire; the traffic therefore on the part of the officers is the same winter and summer.

"From the perfect construction of this railway, sixteen miles of the line being straight, with a medium rise of one in 1028 the whole journey to Zarscoe-Selo will be performed in thirty-five minutes, and to Pawlowsk in forty-minutes. Both places will therefore be converted into suburbs of St. Petersburg, as the time alone fixes the distance. If a passenger is forwarded by the railway for about one and a half rubles, there can be no doubt that as the line runs one and a half miles from the Ligofka to the Fontanka Canal in the interior of the town, a great traffic may be calculated upon.

"In Belgium the railways commence without the towns; in order therefore, to get from Brussels to Malines, it is necessary to ride in an omnibus from the interior of the town to the railway station without and there await the starting of the train, which certainly in thirty or forty minutes arrives at the Malines station; but that being also without the town, it is necessary to get into another omnibus to drive into the town, and in this manner as much time is taken up as by the journey on the railway itself.

"On the St. Petersburg railroad the starting place is nearly in the centre of the city, and it penetrates into the heart of the park of Pawlowsk where the buildings for the reception of the public, stand. As on the railway from Brussels by Malines to Antwerp, upwards of 100,000 passengers pass monthly, and as is well known not one-fourth of them travel on business, the rest for pleasure and recreation, a very considerable traffic must also be expected on the Petersburg line. Nevertheless the Chevalier von Gerstner took in his first estimate of 20th March last year, the number of passengers who would annually travel to Zarscoe-Selo and Pawlowsk and back again, only at 300,000.

"The population of St. Petersburg, Zarscoe-Selo, and Pawlowsk amounts to half a million, that of Brussels, Malines, and Antwerp is not half so much; there we have 1,200,000 travellers annually, here we only calculate upon 600,000, who are to make the journey once; or, in proportion to the population, only one-fourth part of the passengers on the Belgian lines. At this rate, supposing the whole 17,500 shares were issued, there would result a net profit of eleven per cent. to the holders."

The remaining portion of the work treats of a proposed extension of the railway to Ischora, the first station on the road to Moscow, being a preparatory measure to a railway connecting the modern with the ancient capital of Russia—certainly a most gigantic undertaking. It also contains certain terms on which the Chevalier von Gerstner offers to farm the Zarscoe-Selo Railway for three years—a proposition showing the strong confidence he entertains in the remunerative results which will arise from it. Lond. Min. Jour. & Com. Gaz.

Mechanics' Register.

¶ *Description of Mr. Cocking's Parachute and a narrative of the circumstances attending his fatal descent.*

Considerable interest existing at the present moment on the subject of aërial navigation—an interest which has lately been painfully excited by the accident which has befallen Mr. Cocking—the following drawing and description of the Parachute of the unfortunate experimenter, and narrative of his descent, cannot fail to be acceptable to our readers.

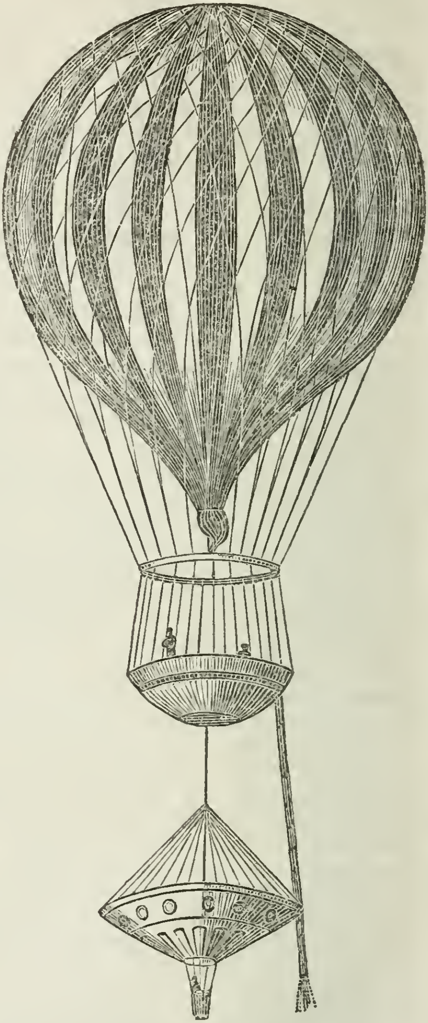
Mr. C. is the first since the day of Garnerin who has had the temerity to venture a descent in a Parachute. On the occasion of the descent of the last named æronaut, about thirty years ago, it was remarked by many scientific men present, that the great oscillation which took place from the moment the machine expanded till it reached the earth, must have been attributable to some defect in the principle of its construction.

Mr. Cocking, a gentleman who has made the science of aërostation his study for many years, and who has also delivered public lectures on the subject, was present at Garnerin's descent, and has since devoted much time and labour to discover the errors in the parachute then employed. Like all inventors, he met with numerous failures and disappointments, but at length his efforts were crowned with success, at least as far as experiments could prove; and we believe the circumstance which first led him to adopt the form now about to be used, was the accidental dropping an umbrella from a balcony. The umbrella fell the first few feet with the handle downwards, but after several oscillations its position was reversed, and having become inverted, it fell steadily to the ground.

Mr. Cocking from this occurrence felt convinced that the proper shape of such an instrument was exactly opposite to that used by Garnerin, and that it should be a convex, instead of a concave surface, or a cone with its apex downwards.

Garnerin's parachute was made of merely a circular piece of canvass, with cords attached at short intervals around the outer edge, all coming to one point below, to which the car was suspended. Now this altogether weighed so little, that any common balloon was capable of ascending with it; but to keep the new instrument in its proper shape, numerous large hoops, wooden braces, and a variety of other apparatus, were necessary, which rendered the whole of so great a weight, that the persevering inventor, after all his labours, saw his model lie useless for years.

On learning the intention of the proprietors of Vauxhall to build a balloon of extraordinary dimensions, Mr. Cocking perceived an opportunity for introducing his parachute, and lost no time in communicating to them his plan. All difficulty was, however, not removed, for the natural question arose as to what was to become of the balloon after the parachute should be detached from it, the custom having previously been to construct one of cheap materials merely to serve for the one ascent, and which was generally either lost or destroyed, having frequently been blown out to sea, for no aëronaut could be found who would undertake to ascend and suffer so great a weight to be instantaneously separated from the balloon. Mr. Green, however, stated his opinion that the object might be accomplished without accident to the aëronaut, and removed all difficulty by offering to make the ascent himself. All was soon arranged, a new parachute of extraordinary dimensions constructed, and Monday, July 24th, fixed for the experiment. Three hoops, the largest of which is 107 feet in circumference, connected by ten light spars of wood, form the frame-work of the machine. These are strengthened by a series of small lines stretching from the upper hoop to the lower, and the whole is covered by a fine cloth; the latter consists of twenty-two gores, 59 inches at their greatest diameters, and gradually diminishing to 11 inches, which, when sewed together, form a cone at an angle of 30°.



The car is of wicker, and its attachment to the lower hoop resembles that of the car balloon, which always retains its perpendicular whatever movement there may take place in the machine above. A strong rope will descend from the hoop underneath the car of the balloon, and passing through an iron ring on the top of the main centre cord of the parachute, will ascend on the other side, and be made fast to the instrument commonly used by Mr. Green for liberating the balloon. From this a thin cord will hang down to the car of the parachute, and thus give Mr. Cocking the opportunity of making the separation at any moment he may deem favourable. The surface exposed to the action of the air is 124 square yards, and the weight of the apparatus 223 lbs. This added to Mr. Cocking's weight, viz., 170 lbs., give the total of 393 lbs.; but it is calculated that the parachute is capable of descending with safety with a weight considerably greater, which is of course all in favour of the success of the experiment. The rate of the descent, as nearly as can be calculated, will be about 10 feet in a second, or six miles and a half an hour; but this will of course, in some measure depend on the state of the atmosphere.

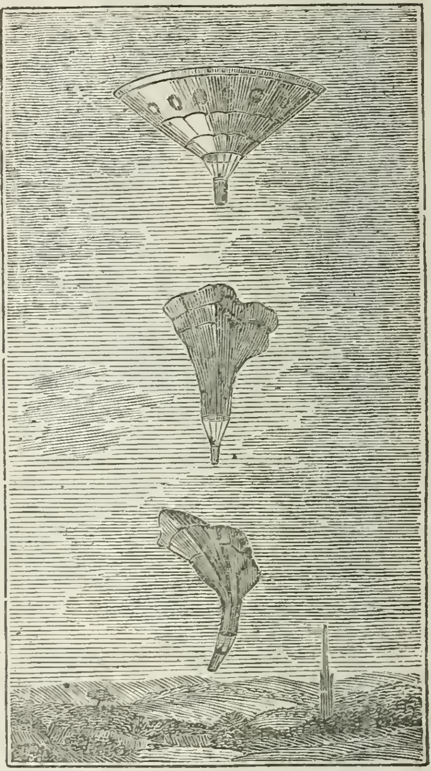
The ascent took place from Vauxhall Gardens, July 24, towards six o'clock, the Messrs. Green entered their balloon, which was allowed to ascend to an altitude of about forty feet, that the parachute might be brought directly under it, and securely fixed. It was seven o'clock before all the preparations were completed, and in about half an hour afterwards, a tube or pipe of linen was lowered by the Messrs. Green from the car of their balloon through the orifice in the parachute, and past the basket in which Mr. Cocking was to sit. This was for the conveyance of the ballast it is found necessary to discharge on the ascent of the balloon, and which, if it had been thrown out in the usual manner would have lodged in the parachute. All the preparations having been completed, Mr. Cocking (having previously stripped off his coat as too cumbersome, and put on a light jacket) stepped into the car amid the acclamations of the company; the cords were loosened, and the balloon and its attendant parachute mounted into the heavens amid the renewed cheering of the crowd. The early part of the afternoon had been remarkably fine and clear, but about this time (half past seven) the sky had become somewhat overcast, and a breeze had sprung up. No apprehensions however, were entertained, and the scene at that moment was as gay and cheerful as it is possible to imagine. The balloon had hardly attained an altitude of two hundred feet when the tube destined for the escape of the ballast from the car above, detached itself by some means or other from the basket of Mr. Cocking, and floated like a riband in the air.

Mr. R. Underwood, of Regent street, followed on horseback in the direction taken by the balloon, to witness, if possible, the descent of the parachute, and from that gentleman we have learned the melancholy details which follow. Mr. Underwood was in the neighbourhood of Blackheath when he saw the cord which attached the parachute to the car severed. The parachute, thus left to itself, descended with the utmost rapidity, and swayed from side to side in the most fearful manner. Mr. Underwood anticipated the worst. In a few seconds, the dreadful oscillations still continuing, the basket which contained the unfortunate aëronaut broke away from the parachute, and Mr. Cocking was precipitated to the earth from a height of several hundred feet. Mr. Underwood immediately spurred his horse, and arrived in a field near Lee, where several labourers had picked up the parachute. They would not believe that a man had fallen with it, but on Mr. Underwood's explanations, and an offer of five guineas to whoever should find the body of Mr. Cocking, they commenced a diligent search. After traversing four fields they heard groans proceeding from a field called Burnt Ash, near Lee, and on going in that direction, they found the unfortunate Mr. Cocking literally dashed to pieces! and just as they were loosening his cravat, he breathed his last in their arms.

When the parachute reached the earth it was a confused heap covering the basket and body of Mr. Cocking, and all the ribs and tubes were broken into fragments.

The accompanying sketch exhibits the parachute at three stages of its descent. First, immediately after the experimenter separated from the car; next, at the time when the collapse took place from the weight and pressure of the atmosphere; and last, when it approached near the ground.

The following is Messrs. Green and Spencer's narrative.



In consequence of the sad and fatal catastrophe which has befallen the late Mr. Cocking, I feel myself called upon to communicate to the public the whole of the particulars of my ascent with the Vauxhall Balloon, taking up with me Mr. Cocking in his parachute. The inflation commenced about twelve under the able direction of Mr. Hutchinson, the engineer of the London Gas Company, and was completed by five o'clock. Prior to the parachute being attached to the balloon I caused a trial to be made with the view of ascertaining whether the buoyancy of the latter was sufficient to carry up the former with safety. The result of this trial was, after some arrangements with respect to the ballast, of which I was compelled to give out about 650 lb. in weight, had been effected satisfactorily. The abandonment of this large quantity of ballast I found to be absolutely requisite in order with safety to commence the ascent. The balloon was then allowed gently to rise a sufficient height to be conveyed over the parachute; but in consequence of the great and unavoidable delay which was necessarily caused in affixing the two machines, the gas in the former became very considerably condensed, from a reduction of temperature. It, thereupon, became a matter of compulsion that I should get rid of 100 lb. more of ballast, which I emptied out of the bags through a tube, constructed of canvass, and about fifty feet in length. The object in having this tube was, that any ballast I might deem it advisable to throw out during our voyage should take such a course as would clear the broadest expanse of the parachute. The connexion between the balloon and the parachute was at length completed by the rope of the latter being made fast to the liberating iron by which Mr. Cocking was to free himself from the balloon.

It is but justice to myself I should here state, that I had on several occasions expressed my determination not to liberate the parachute from the balloon, upon the ground, setting aside any other consideration, that I might select a moment for severance when Mr. Cocking was not altogether prepared or ready for his descent, and therefore if any accident were to accrue to him, that I of course should be regarded as the responsible party, to whom blame would naturally attach.

Every thing being in readiness, about 25 minutes to eight o'clock, Mr. F. Gye, gave the signal for the whole of the apparatus to be released from its trammels, and we instantly rose very steadily, taking an easterly course.

Mr. Cocking always desired that we should ascend to an elevation of 8,000 feet, about one mile and a quarter, at which height he proposed to detach himself from the balloon, and commence his descent. Finding, therefore, that our upward progress was very slow, I requested Mr. Spencer to discharge some more ballast, and he accordingly threw the contents of a bag weighing 20 lb. through the tube already named. This proving of little avail, I directed a second and then a third bagful to be got rid of by the same means.

At this period we were floating over the Surrey Zoological-gardens, at an elevation of about 2,000 feet. It was at this moment that a portion of the lower end of the ballast-tube became detached, a circumstance which was caused by the occasional swinging to and fro of the parachute. This accident led to the inconvenience which I had forseen some days before the ascent, and which led to the adoption of the tube, and of that of rendering it extremely difficult for us to discharge the ballast without its falling into the parachute.

Our inability to do this as we were then situated I communicated to Mr. Cocking, adding that under the circumstances it was impossible for us to rise any higher unless we were to attempt to throw the ballest in bags beyond the outer spread of his machine, a course of procedure which we considered to be attended with much danger to any person who might chance to be beneath, but that we would, if he wished it, make the experiment as soon as he had cleared the houses. Mr. Cocking replied "Very well, it is of no consequence; if you think I have time to rise as high as I want, and to descend before dark." I remarked, "I think you have, and you will then also have a more open country for the descent." We now continued to glide along guided by the pleasure of the wind at nearly the same elevation until we had cleared all buildings. During this time Mr. Spencer and myself were busily engaged in dividing our ballast into small parcels, so that we might be able to throw them over without injury to the parachute.

As soon as we found that we had arrived over the fields, and presuming that no danger could arise from the falling of the ballast, we quickly began to relieve ourselves of that essential commodity. In doing this our anxiety respecting any of it lodging in the parachute was much relieved by finding that that machine continually swung backwards and forwards, evidently occasioned by the operation of the currents through which we passed, so then we were enabled without any difficulty to cast away the bags without damage to the vehicle immediately below us. We continued to discharge ballast until we had lessoned our quantity by 50lb., in addition to that already sent over. The balloon now began to rise, and soon entered a tier of clouds, when we lost sight of the earth. So great, however, was the resistance offered by the parachute to this denser atmosphere that we were again obliged, in order to attain the elevation Mr. Cocking pressed for (that gentleman considering that the greater the distance he had to fall, the greater would be the atmospheric pressure under the parachute, and therefore the easier descent) to rid ourselves of 400lb. more ballast, and even then, we only arrived at the height of 5,000 feet, which is a trifle less than a mile. We were still 3,000 feet lower than Mr. Cocking's desired elevation.

"Whilst these operations were going on, Mr. Spencer and myself held a conversation with our appended neighbour and friend, which was entirely confined to the progress we were making upwards, Mr. Cocking manifesting much anxiety, and wishing to be informed how we were rising, requesting to know when every additional elevation of 500 feet was accomplished.

As soon as we had attained the height of 5,000 feet I told him that it would be impossible for us to get up as high as he desired in sufficient time for him to descend by the light of day. Upon this Mr. Cocking said, "Then I shall very soon leave you; but tell me whereabouts I am!" Mr. Spencer, who had a few minutes before caught a glimpe of the earth, answered, "We appear to be on a level with Greenwich." I then asked him if he felt himself quite comfortable, and whether he found that the practical trial bore out the calculation he had made? Mr. Cocking replied, "Yes; I never felt more comfortable or more delighted in my life." Shortly afterwards Mr. Cocking said, "Well, now I think I shall leave you." I answered, "I wish you a very good night and a safe descent, if you are determined to make it, and not to use the tackle."

I should here observe, that with an anxiety to prevent any accident arising in the event of the violence of the wind rendering it impossible for a descent to be attempted, an apparatus had been constructed under the direction of Mr. F. Gye, to afford us the facility of assisting Mr. Cocking to haul himself up into the car of the balloon, and that this is the tackle to which I thus alluded.

Mr. Cocking to this question made no other reply than "Good night, Spencer; good night, Green."

At this instant I desired Mr. Spencer to take fast hold of the ropes and like myself to crouch down in the car. In consequence of being compelled to keep hold of the valve line, of course I had but one hand which was available for the purpose of safety. With that hand, fortunately, in the perilous situation into which we were speedily thrown, I was able to maintain my position.

Scarcely were these words uttered before we felt a slight jerk upon the liberating iron, but quickly discovered, from not having changed our elevation, that Mr. Cocking had failed in his attempt to free himself. Another but more powerful jerk ensued, and in an instant the balloon shot upwards with the velocity of a skyrocket.

The effect upon us at this moment is almost beyond description. The immense machine which suspended us between "heaven and earth," whilst it appeared to be forced upwards with terrific violence and rapidity through unknown and untravelled regions, amidst the howlings of a fearful hurricane, rolled about as though revelling in a freedom for which it had long struggled, but of which until that moment it had been kept in absolute ignorance. It at length, as if somewhat fatigued by its exertions, gradually assumed the motions of a snake working its way with astonishing speed towards a given object. During this frightful operation, the gas was rushing in torrents from the upper and lower valves, but more particularly from the latter, as the density of the atmosphere through which we were forcing our progress pressed so heavily on the valve at the top of the baloon as to admit of comparatively but a small escape by that aperture.

At this juncture, had it not been for the application to our mouths of two pipes leading into an air-bag with which we had furnished ourselves previous to starting, we must within a minute have been suffocated, and so, but by different means, have shared the melancholy fate of our friend.

This bag was formed of silk, sufficiently capacious to contain 100 gallons of atmospheric air. Prior to our assent the bag was inflated, with the assistance of a pair of bellows, with 50 gallons of air, so allowing for any expansion which might be produced in the upper regions. Into one end of this bag were introduced two flexible tubes, and the moment we felt ourselves to be going up, in the manner just described, Mr. Spencer, as well as myself, placed either of them in our mouths. By this simple contrivance we preserved ourselves from instantaneous suffocation, a result which must have ensued from the apparently endless volume of gas with which the car was enveloped. The gas, notwithstanding all our precautions, from the violence of its operation on the human frame, almost immediately deprived us of sight, and we were both, as far as our visionary powers were concerned, in a state of total darkness for between four and five minutes.

As soon as we had partially regained the use of our eyes, and had somewhat recovered from the effects of the awful scene into which, from the circumstances, we had been plunged, our first attention was directed to the barometer. I soon discovered that my powers had not sufficiently returned to enable me to see the mercury, but Mr. Spencer found that it stood at 13-20, giving an elevation of 23,384 feet, or about four miles and a quarter.

I do not conceive, from the length of time I had been liberating the gas that this was any thing like our greatest altitude for we were evidently effecting a rapid descent. This impression is corroborated by a rough calculation, which leads me to believe, knowing the customary rate at which the gas makes its escape, taken in consideration in conjunction with the length of time I had been pulling the valve-line, that we had lost at least 30,000 feet of gas or 180,000 gallons, a total of 5,000 feet more than my own balloon will contain.

It may be regarded as somewhat surprising that not a larger quantity had evaporated, especially when the size of the valves are considered, that at the top being nearly three feet in diameter, whilst the one at the neck of the balloon is upwards of two feet. The extreme rapidity with which we ascended, coupled with consequent pressure of the atmosphere on the upper part of the machine, necessarily prevented much escape from the top valve. The same cause also forced an extraordinary emission from the opening at the neck, and I am decidedly of opinion, had it not positively happened that the proprietors permitted this latter valve to be increased from 18 to 25 inches in diameter, that the baloon must have burst, and my companion as well as myself, been hurled headlong into eternity.

As I have stated, we were now rapidly on the descent, having got rid of all the unusual annoyances to which I have referred ; and finding that we were proceeding downwards with the ordinary calmness and steadiness, although with much speed, we hastened to empty two tin vessels of water which we had taken up for the purpose, and to charge them with the atmospheric air through which we were then descending. Our desire was to effect this object at our greatest altitude, but from the circumstances which I have detailed we were unable to accomplish that end, and when the vessels were filled, the mercury in the thermometer had ascended to 17-50, or an elevation of 16,632 feet, about three miles.

When we had accomplised this matter, finding ourselves suffering severely from cold, we referred to the thermometer, which stood at 28, four degrees below the freezing point.

We were at this period apparently about two miles and a half above a dense mountain of clouds, which presented the appearance of impenetrable masses of dark marble, whilst all around us were shed the brilliant rays of the setting sun. We continued to descend with great rapidity, and as we approached the clouds, that velocity considerably increased. At this time so large had been our loss of gas, that the balloon, instead of presenting to our sight its customary round and widely expanded form, now merely looked like a comparatively small parachute, or half dome, without any aperture in its centre. We had parted with at least one-third of our gas, and were as far beneath the balloon itself as 50 or 60 feet.

Recollecting the late hour at which we quitted Vauxhall, I now began to be anxious about the time, and on applying to Mr. Spencer, ascertained that it wanted not more than a quarter of 9 o'clock. From this I was aware, notwithstanding in our then position we were blessed with a magnificent light, that on emerging below from the clouds darkness would have assumed her sable hue over the earth, and that we should have much difficulty, therefore, in ascertaining the nature and character of the country, supposing us to be over the land, on which we must effect our final descent. I, consequently, became extremely anxious to make our way through the clouds as quickly as possible, which having done we proceeded, until we had reached within some 300 feet of the ground, when we found it requisite, from our inability to ascertain the nature of the ground, the whole country beneath us offering the appearance of thick woods, to cast out every article of ballast and movable matters, even to ropes and empty ballast-bags, in order to prevent us from coming in contact with what was supposed to be trees. After calling out for some time, and hanging out the grapnel, we heard voices in reply, and the parties speedily drew us to a safe place of landing, which proved to be close to the village of Offham, near Town Malling, seven miles west of Maidstone, and 28 from London.

The balloon was packed, and conveyed in a cart to Town Malling, where we were most hospitably treated and provided with beds by the Rev. Mr. Money, who, singular to relate, informed me that he is the son of Major Money, the aeronaut, who, on the 23d July, 1785, ascended from Norwich, and fell into the sea 20 miles off Lowestoff.

At half-past ten o'clock this morning we quitted Town Malling, and it was not until our arrival at Wrotham, at which place I inquired whether they had heard where Mr. Cocking had descended, that I became acquainted with the unexpected and melancholy result of his experiment.

I trust it is needless for me to say, how deeply the feelings of Mr. Spencer and myself were harrowed up by the sad intelligence thus conveyed to us.

It is only due to the late Mr. Cocking I should add, that throughout the whole of our voyage, up to the moment when he released himself from the balloon, he displayed the greatest courage and fortitude, and the expression of his features, and the light and joyous, although earnest way in which he made his inquiries and conversed with us, manifested his great satisfaction that at length a theory to which he had devoted the last 25 years of his life was about to be triumphantly put to the test.

We were up about one hour and twenty minutes.

Individually my opinion was, that having withstood the difficulties and severe pressure of the atmosphere in its ascent, Mr. Cocking's parachute would accomplish its descent with perfect safety."

The following evidence of Prof. Airy, before the coroner's inquest, will furnish all the additional information of interest in a scientific point of view.

Professor G. B. Airy, the Astronomer Royal of Greenwich, stated that he saw the balloon shortly after its starting, it was then travelling with the wind. It shortly afterwards altered its course. The parachute he saw, and was struck with its appearance. He saw it through a common telescope of a 12-times magnifying power. He was sitting with some ladies of his family at the drawing-room window, looking at the balloon; suddenly he heard a loud shout of persons in the park, and he knew that the parachute had got away. It had been stated that the parachute kept its shape for 40 or 50 seconds, but he was quite sure that it did not retain its shape for more than four seconds, for he put his eye instantly to the glass and found it in a collapsed state. He was convinced there had been no turning over. Had it been turned over the basket would have been displaced. He observed the sides of the parachute flickering backwards and forwards. His opinion as to the efficacy of the construction was, there was not sufficient account taken in such construction as to unavoidable disturbances, and the tendency of the air was to force it in at the side, and the pressure of the air would in case of its getting out of shape only aggravate the evil, and the experiment must fail. This must therefore be considered as a construction quite wrong, and he should have thought that a person with common sagacity might have been aware of this. With regard also to the tin tube of which the circular ring was formed, it was hollow throughout, it was without stops, which would have strengthened it, and consequently as bad a thing as could have been used. Had stops been introduced it would have saved it from a great deal of the tremor to which the pressure of the atmosphere exposed it. Had the weight been a little greater in the top it would probably have come down sideways, and turned upside down. In this respect it was very badly constructed, and very inferior in many respects to parachutes of the old construction.

A jurer said it had been considered that if it resisted the force of the atmosphere in ascending it would be perfectly safe to come down with it. He wished to know what was the professor's opinion on this point?

Professor Airy said his opinion was quite the reverse. The air, by pressing upon the canvass, would keep the ring of tin to which he had alluded expanded, but the force of the air under would have the effect of bending it, and thus allowing the parachute to collapse.

LUNAR OCCULTATIONS FOR PHILADELPHIA, OCTOBER 1837.

Angles reckoned to the right or westward round the circle, as seen in an inverting telescope.
☞ For direct vision add 180° ☜

Day.	H'r.	Min.	Phase	Star's name.	Mag.	from Moon's North point.	from Moon's Vertex.
7	6	59	Im.	b, Saggittarii,	5,	70	74
7	8	0	Em.			328	344
15	10	52	Im.	65 Arietis,	6,	112	62
15	12	2	Em.			311	270
31	4	11	Im.	d Scorpii	1,	94	123
31	5	25	Em.			250	290

LUNAR OCCULTATIONS IN NOVEMBER, 1837.

Day.	H'r.	Min.	Phase	Star's name.	Mag.	from Moon's North point.	from Moon's Vertex.
11	4	52	Im.	π Arietis	,5,	146	96
11	5	37	Em.			267	214
11	8	6	Im.	ρ^2 Arietis	,6,	146	94
11	9	7	Em.			280	233
14	11	13	Im.	c Tauri	,4.5,	43	344
14	11	51	Em.			343	286

Meteorological Observations for June, 1837.

Moon.	Days.	Therm.		Barometer.		Wind.		Water fallen in rain.	State of the weather, and Remarks.
		Sun rise.	2 P.M.	Sun rise.	2 P.M.	Direction.	Force.		
				Inches	Inches			Inches.	
	1	64°	82°	29.85	29.96	SW.	Moderate.		Partially cloudy—do.—auro. bor.
	2	62	85	96	96	SW.	do.		Lightly cloudy—clear, aur. bor.
⊗	3	66	86	75	60	S.	Brisk.	.3	Clear—shower.
	4	67	76	55	56	W.	do.	.28	Clear—cloudy—thunder shower.
	5	62	74	00	00	W.	do.		Partially cloudy—do. do.
	6	59	82	65	65	W.	Moderate.		Clear—do.
	7	64	84	50	45	W.	do.		Clear—do.
	8	60	62	45	55	NE.	do.		Cloudy—do.
	9	61	72	70	80	E.	do.		Cloudy—do.
	10	54	74	86	90	E.	do.		Cloudy—do.
☾	11	55	68	95	90	E.S.	do.		Cloudy—do.
	12	64	72	90	84	S.	do.		Cloudy—do.
	13	69	72	75	75	S.NW.	do.	.14	Rain—partially cloudy.
	14	58	79	75	80	S.	do.		Partially cloudy—clear.
	15	58	63	70	70	N.	do.	.25	Cloudy—rain.
	16	54	74	70	72	W.	Brisk.	.17	Clear—do.—shower.
	17	52	70	72	72	NW.	do.	.5	Partially cloudy—shower.
🌝	18	54	67	70	80	NW.	do.		Cloudy—flying clouds.
	19	51	69	80	80	SW.	do.	.80	Clear—cloudy—rain in night.
	20	60	71	00	00	W.	Moderate.		Cloudy—flying clouds.
	21	60	72	00	5	W.	do.	.10	Rain—flying clouds.
	22	58	77	10	75	W.	do.		Flying clouds—clear.
	23	58	77	30.00	30.00	S.	do.		Cloudy—lightly cloudy.
	24	66	70	29.90	29.85	S.	do.		Cloudy—rain.
☽	25	69	77	77	77	W.	do.	.30	Cloudy—shower.
	26	63	81	90	95	W.	do.		Fog—lightly cloudy.
	27	66	82	95	80	W.	do.	.7	Cloudy—shower.
	28	68	80	90	30.00	W.	do.		Cloudy—do.
	29	66	68	30.00	00	S.	do.	.45	Cloudy—rain.
	30	64	86	29.80	29.80	W.	do.	.2	Cloudy—clear—rain.
	Mean	61.06	75.06	29.68	29.71			2.66	

	Thermometer.	Barometer.
Maximum height during the month	86. on 3d & 30th.	30. on 23d 28th 29th.
Minimum do.	51. on 19th	29. on 5th 20th 21st.
Mean do.	68.06	29.69.

JOURNAL
OF THE
FRANKLIN INSTITUTE
OF THE
State of Pennsylvania,
AND
MECHANICS' REGISTER.
DEVOTED TO
Mechanical and Physical Science,
CIVIL ENGINEERING, THE ARTS AND MANUFACTURES,
AND THE RECORDING OF
AMERICAN AND OTHER PATENTED INVENTIONS.

OCTOBER, 1837.

Practical and Theoretical Mechanics and Chemistry.

On Hydraulic and Common Mortars. By General Treussart, *Inspecteur du Génie. Translated from the French by J. G. Totten, Lt. Col. of Eng. and Brevet Col. United States Army.*[*]

Art. 1. *On the present state of our knowledge of Lime.*[†]

Lime has been employed from time immemorial. Mixed with sand, or certain other substances, it forms what is called mortar. Although the solidity and durability of masonry depends on the goodness of mortar; still, few experiments have been made with lime; and the manner of making mortar has almost always been given up to workmen. It is only within

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[*] Copy right secured.

[†] The present article is the first of a series on hydraulic and common mortars. In presenting them, the Translator thinks that he is rendering a service to the art of construction in this country.

The French, within a few years, have devoted themselves with great zeal and effect to the subject of mortars; and they have applied, with perfect success, the results of their experimental investigations, to actual constructions of great difficulty and importance. The English are, perhaps, equal to the French in knowledge of the best constituents of mortars, and in the practical composition and use of them; but they are not, like their neighbors, in the habit of recording, methodizing, and publishing their experience: and hence, though important information may be very generally spread about fifty years that a few scientific men have attended to this important subject. Comparing the mortars of the ancients, and especially of the Romans, with those of modern times, it was perceived that the old mortars were much better than ours; and the means have, consequently, been sought of imitating them. Several constructors have thought they had discovered the secret of making Roman mortars: others, on the contrary, have thought that the Romans had no particular process, but that, of all their constructions, those only which were made of good lime had survived to our day. We shall see that my experiments tend to confirm this latter opinion.

Lime used in building, is obtained by the calcination of calcareous stones, which occur abundantly on the surface of the globe. Marbles, certain building stones, chalk, calcareous alabaster, and shells, are employed in making lime. The effect of calcination is to drive off the water and the carbonic acid which are combined with the lime. The water and the first portions of carbonic acid pass off easily; but it requires an intense, and long continued heat to dispel the remainder of the acid. Lime, as used in constructions, contains, almost always, a considerable quantity of carbonic acid.

When the stone submitted to calcination is white marble, pure lime is obtained, provided the calcination be carried far enough. According to an analysis which I made of white marble, this substance contained, in 100 parts, as follows: lime 64; carbonic acid 33; water 3. Lime obtained by calcination possesses the following properties. It has a great avidity for water, imbibes it from the air, and has its bulk enlarged thereby. If a certain quantity of water be thrown on lime recently calcined, it heats highly, breaks in pieces with noise, and a part of the water is evaporated by the heat produced. The disengaged vapour carries off some particles of lime. Water dissolves about one four-hundredth of its weight of lime, forming what is called lime-water.* Lime is caustic and turns the syrup of violets,

among their builders of all classes, it is no where to be found concentrated in books.

In this country we have been led, within a few years, to some improvements in the practice of mortar-making, by the actual necessity imposed by extensive hydraulic works, and by the providential diffusion over our territory of an admirable material; still we are, for the greater part, the slaves of an antiquated routine, elsewhere known to be radically wrong.

The works here translated are recent, and amongst the best issued from the French press: others have been carefully consulted; as Vicat's, Raucourts's, Soleirol's, &c., but these have been preferred as answering best to our need. To most American readers, they will present many new results, highly important, and of a character to be applied with great advantage to our own operations—they are, therefore, commended to the persual of all who have an interest in the subject.

In relation to the terms employed in translation, it is proper to state that the expression *fat lime*, used for the *chaux grasses* of the French, is applied to lime nearly or quite pure, not hydraulic, and which swells much in slaking. *Poor lime*, or *meagre lime*, substituted for *chaux maigres*, means a lime which slakes reluctantly, swells but little, and is not hydraulic. *Hydraulic lime, chaux hydrauliques,* implies a lime which will harden, in a longer or shorter time, under water—this generally slakes slowly and swells but little. The French term *ciment* means, almost always, finely pounded bricks or tiles: with us, *cement* is a sort of generic term, having various significations, but generally implying, when used herein, the hydraulic constituent of mortars

The term *Puzzalona* belongs, strictly, to a peculiar volcanic product, formerly much used in hydraulic mortars; and which is particularly referred to within; it is, however, often used to signify natural substances not volcanic, and even artificial substances, if producing effects similar to those afforded by the real *puzzalona.*

The English having adopted the expressive term *concrete*, to signify the mixture called by the French *beton*, the same term will be used in this translation.

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* One four hundredth, *Davy*—one seven hundredth and fifty-eighth, *Thompson*—one seven hundredth and seventy-eight, at 60° Fahr., *Dalton*—Tr.

green: its specific gravity, according to Kirwan is 2.3, it attracts carbonic acid from the air, and finally returns to the state of carbonate of lime. To preserve it, it is necessary to keep it in very tight vessels.

Lime was formerly ranked among the alkalis, and it is only lately that the true nature of the substance became known. Davy, the English chemist, succeeded in 1807, in decomposing, by means of Volta's pile, the sulphate, and the carbonate, of lime, or more properly *lime* derived from these compounds; obtaining a brilliant substance, having so strong an attraction for oxygen that it absorbs it rapidly from the air, and from water, which it decomposes. The brilliant substance obtained from lime is regarded as a metal and has received the name of Calcium. Accordingly, lime is only a metallic oxide.

It is rare that lime derived from white marble is used in the arts; that which is commonly employed, and which is derived from ordinary lime stone, almost always contains oxide of iron, and sometimes a certain quantity of sand, alumine, magnesia, oxide of manganese, &c. Some of these substances combine with the lime by calcination: and the lime thus acquires properties which it had not before, and of which I shall speak in the sequel.

If we take lime derived from white marble, or from common lime stone, and reduce it as it comes from the kiln, to a paste with water, and if we place this paste in water, or in humid earth, it will remain soft forever. The same result will be obtained, if lime be mixed with common sand and the resulting mortar be placed in similar situations.

It is a common practice to deluge lime, fresh from the kiln, with a large quantity of water, and run it into large basins, where it is allowed to remain in the condition of soft paste. Alberti says (book II., chap. XI.) he has "seen lime, in an old ditch, that had been abandoned about 500 years, as "was conjectured from several manifest indications; which was still so moist, "well tempered, and ripe, that not honey or the marrow of animals could be "more so."

There is another kind of lime which possesses a singular property: if it be slaked as it comes from the kiln, as above, and be then placed in the state of paste, in water, or in moist earth, it will harden more or less promptly, according to the substances it contains. The same result is obtained if the lime, being mixed with sand, is made into mortar and placed in similar situations. If this lime be slaked and run into vats, as is done with common lime, it will become hard after a little time, and it will then be impossible to make use of it.

On slaking lime, fresh from the kiln, with enough water to reduce it to paste, it is found to augment considerably in bulk; this augmentation is such that one volume of quick lime will sometimes yield more than three volumes, measured in the condition of thick paste. When lime which has the property of hardening in water is slaked in the same manner, it affords a much smaller volume than common lime. Sometimes one volume of this lime, measured before slaking, will give, when slaked to thick paste, scarcely an equal bulk. For a long time, those limes which had the property of hardening in water were called *meagre limes*, and those which had not this property were called *fat limes*. These denominations were affixed because the first kind increased but little in bulk when made into paste, while the other give a considerable augmentation of volume; and because fat limes formed, with the same quantity of sand, a mortar much fatter or more unctuous than meagre lime. But the designation "*meagre lime*" is altogether improper to indicate limes which enjoy the property of hardening in water; because there are limes which augment their volume very little, on being made into paste, and at the same time possess no hydraulic property. Belidor gave the name of béton to lime which had the quality of hardening in water; but many engineers continued to call it *meagre lime.* The denomination of *béton* is not suitable; and, in this sense, is not now in use. The following are the terms now employed.

In England, the name of *aquatic lime* has been given to lime which indurates in water; in Germany it is called *lime for the water*; Mr. Vicat, Engineer of roads and bridges, has proposed the name of *hydraulic lime*, and this denomination, which is a very good one, has been generally adopted. I shall therefore call that lime which swells considerably in slaking, *fat lime*, that which swells but little and does not harden in water, *meagre lime*, and that which possesses the property of hardening in water, *hydraulic lime. Fat lime* is often called *common lime*, also. The term *quick lime* is applied to all unslaked limes whether *fat lime*, *meagre lime*, or *hydraulic lime.* Although *meagre lime and hydraulic lime* may have been calcined exactly to the proper degree, still they are slower to slake, and give out less heat than *fat lime.* When *fat lime* has been too much burned, it, also, becomes slow to slake; while, if properly burned, it begins to slake the instant water is thrown on. Experiments, to be given in the sequel, will show that iron, in the state of red oxide, causes *fat lime* to slake sluggishly.

Some of the ablest chemists have, at different times, sought to detect the substances which impart to lime the property of indurating under water.

Bergman, a Swedish chemist, was, I think, the first who gave an analysis of a hydraulic lime-stone. That from Léna in Sweden, he found to contain, in 100 parts, the following substances; lime, 90; oxide of manganese, 6; clay, 4. Bergman seems to have attributed the peculiar property of hydraulic lime, to the oxide of manganese; and this opinion prevailed for a long time. On the other hand we find in the *Bibliothèque Britannique* of 1776, vol. 3, page 202, that Smeaton, the English Engineer, who built the Edystone Light-house, in 1757, attributed this property to clay: for he says that it is a curious question, which he leaves to chemists and philosophers to decide, why the presence of clay in the tissue of a calcareous stone should give it the property of hardening in water, while clay added to common lime produces no such effects.

Guyton de Morveau, announced in a memoir published in the year 9, that he had detected the presence of oxide of manganese in all the lime stones which afforded hydraulic limes; he announced, further, that in calcining together 90 parts of common lime stone pulverized, 4 parts of clay, and 6 parts of black oxide of manganese, an excellent artificial *meagre lime* would be obtained. It was stated above, that at that time, the name *meagre lime* was given to lime that would set under water; the French chemist was the first therefore to make artificial hydraulic lime; but he, as well as Bergman, was mistaken in supposing that the presence of the oxide of manganese was necessary to the result. He would have obtained his result by burning the pulverized lime stone with clay alone.

Mr. Saussure, in his *Voyage des Alpes*, says that the property possessed by certain limes of hardening in water is due solely to silex and alumine (that is to say, to clay) combined in certain proportions.

Mr. Vitalis, chemist of Rouen, made, in 1807, the analysis of the limestones of Senonches and St-Catherines, near Rouen; the analysis is contained in the memoir on the schists of Cherbourg (page 58) published in 1807, by Mr. Gratien, Sen., engineer of roads and bridges. This lime stone contains, according to Mr. Vitalis, in 100 parts, the following substances, water, 12; carbonate of lime, 68; alumine, 12; sand, 6; oxide of iron, 2. In addressing these results to Mr. Gratien, senior, Mr. Vitalis expresses himself thus: "It follows from the analysis that the lime-stones of Senonches and St-Catherines, are two calcareous marles, in which the chalk predominates it is true, but wherein the clay performs an important part. It is this portion of clay which, in my opinion, makes the lime of these two lime-stones, *meagre*; whence it follows that the presence of oxide of manganese is not indispensable to the constitution of such limes, since the analysis proves that the lime-stone in question contains no oxide of manganese, as it would, if present, have coloured the glass violet." I noticed above that these hydraulic limes were then called *meagre limes.* We see that the analysis of these stones confirms the opinion of Mr. Saussure, who had attributed to the clay alone, the property of hardening in water. Thompson, an English chemist, was of the same opinion.

Mr. Descotils, engineer of mines, also made an analysis of the lime-stone of Senonches; which analysis may be found in the *Journal des Mines* of 1813, page 308. According to this trial, the Senonches lime-stone contains a quarter part of silex, disseminated in very fine particles, and only so small a quantity of iron and alumine, that these substances can have no influence on the lime; whence this engineer concludes that the hydraulic property of this lime-stone is owing to the silex. We have, however, seen above, that, according to Mr. Vitalis, it contains twice as much alumine as silex. Mr. Berthier also inserted in the *Journal des Mines* an analysis of the Senonches lime-stone, which will be given further on, and according to which the stone contains very litte alumine. This contradiction has not yet been explained. Perhaps the quarries at that place afford stones of different kinds. If so, it would be important to ascertain what is the composition of the best.

The analysis of the Senonches lime-stone afforded Mr. Descotils occasion to make an important remark on the silex contained in lime-stone: namely, that the silex found in these stones does not dissolve in acids before calcination, but does dissolve after calcination. This fact proves that the properties of silex are changed by calcination with lime, and that it combines in the dry way with this substance.

Mr. Vicat, engineer of roads and bridges, published in 1818 a very important memoir on hydraulic mortars. This engineer set out with the opinion generally admitted at that time, that it was the clay which gave to lime the singular property of hardening in water. He, in consequence, took fat lime, which he mixed with various proportions of clay, according to the following process, extracted from page 7. "The operation we are about to describe (says Mr. Vicat) is a true synthesis, reuniting in an intimate manner, by the action of fire, the essential principles which are separated from hydraulic lime, by analysis. It consists in allowing the lime, which is to be improved, to fall spontaneously to powder in a dry and covered place; afterwards to mix it, by the help of a little water, with a certain quantity of gray or brown clay, or simply with brick earth, and to make balls of this paste, which, after drying, are to be burned to the proper degree.

"Being master of the proportions, we may concieve that the factitious lime may receive any degree of energy desired, equal to, or surpassing at pleasure, the best natural lime.

"Very fat common lime will bear 0.20 of clay to 1.00 of lime; moderately fat lime will have enough clay with 0.15; and 0.10, or even 0.06, of clay will suffice for these limes which are already somewhat hydraulic. When the proportion is forced to 0.33 or 0.40, the lime does not slake, but it pulverizes easily, and gives, when tempered, a paste which hardens under water very promptly."

Such is the process indicated by Mr. Vicat. But this engineer did not content himself with experiments on a small scale: a manufactory was established near Paris by his means, where artificial hydraulic lime is made in large quantities; he moreover exerted himself to extend the use of hydraulic mortar every where, and he succeeded. He has, therefore, rendered an important service to the art of construction, and I have done him the justice to make this acknowledgment, in the notices I have heretofore published.

In 1818, Dr. John, of Berlin, presented to the Society of Sciences in Holland, a memoir, which was published in 1819. This memoir, crowned in 1818, by the Society, answered the following question proposed by the Society: "What is the chemical cause, in virtue whereof stone lime makes generally more solid and durable masonry than shell lime, and what are the means of improving shell lime in this respect?" Dr. John has remarked that shells require to be more highly calcined than common lime-stone: he thinks this owing to the shells being purer carbonate of lime than common lime-stone, which contains earthy substances facilitating the disengagement of the carbonic acid. In making the analysis of sundry lime-stones, he found that those which afforded hydraulic lime contained clay, oxide of iron, &c. He called the foreign matters which gave the property of hardening in water, *cements*; and says that it is possible, by introducing cement in the dry way to ameliorate lime which contains none. On these considerations he made the following experiments. He mixed the powder of oyster shells, 1st, with $\frac{1}{16}$ of silicious sand—2d, with several proportions of clay, varying from $\frac{1}{10}$ to $\frac{1}{3}$—3d, with $\frac{1}{40}$ of oxide of manganese. He tempered these mixtures with water, formed them into balls, let them dry in the air, and then burned them in a lime-kiln for 96 hours. The following results were obtained: the first mixture was agglutinated but friable, and was not a good result; the second mixture gave good results; and the third possessed no peculiar property. The author concludes that clay is the ingredient which gives to common lime the property of hardening in water; and he says that nothing can be easier than to procure good hydraulic lime, either from shells or from pure lime-stone, following the process indicated: he adds that it is for constructors to determine the best mixture to be made in each case.

The memoir of Dr. John contains the analysis of several ancient mortars; and offers several important observations of which I shall have occasion to speak.

In the third number of the *Annales des Mines* of 1822, there is a very interesting memoir by Mr. Berthier, *Ingénieur en Chef des mines*; it contains the analysis of different lime-stones, and several new views which will contribute to form a more perfect theory of mortars. I shall have more than one occasion to cite his experiments, and his opinion on several important facts.

Mr. Raucourt, engineer of roads and bridges, published at St. Petersburgh in 1822, a work wherein he narrates the experiments he made, following the process used by Mr. Vicat, and adding several of his own. Mr. Bergère, *chef de bataillon du génie*, gave an analysis of this work, in the *Annales des Mines* of 1824, Vol. IX.

In 1825, Mr. Hassenfratz published a memoir on mortars. This work, which is voluminous, contains many practical details on the calcination of lime-stone in different countries, and exhibits the actual state of knowledge, in the art of making mortars at the period of publication.

In terminating this reference to works on hydraulic mortars, which have appeared up to this time, I must introduce a fact, entirely new, announced by Mr. Girard de Caudemberg, engineer of roads and bridges, in a notice published by him in 1827. He states that the proprietors of mills on the river Isle, in the department of Gironde, discovered by accident, a kind of fossil sand to which they gave the name of *aréne*, which has the singular property, without any preparation, of forming, with fat lime, a mortar that hardens under water, and has great durability. I shall have occasion to return to this important fact, and to report what Mr. Girard says, as well as to state the principal experiments which have been made with this substance, in other places where it has been found.

I was employed from 1816 to 1825 at Strasburg, at which place they had made no use of hydraulic lime. I ascertained, however, that such lime was to be found in the neighbourhood. Almost all the hydraulic works connected with the fortifications of the place, having been badly constructed, and dating as far back as Vauban's time, were to be rebuilt. Twenty-five years' experience had taught me the great superiority of hydraulic mortars in the air as well as in the water—where, indeed, they are indispensable. I tried, therefore, the hydraulic limes, afforded by the environs of Strasburg, and found them excellent: they were, consquently, used in all the works both in air and water. All the revetments built from *port de Pierre* to *port Royal*, having a development of about 1650 yards, were rebuilt or repaired with hydraulic mortar. It was the same with the hydraulic works; they were rebuilt or repaired with the hydraulic lime of the neighbourhood.

An engineer who should use fat lime, even for constructions in the air, when there are hydraulic limes at hand, would be very censurable, because the expense is about the same, and, as regards the strength and durability of masonry, there is a vast difference in favour of the hydraulic mortar. But in countries where no hydraulic lime is to be had, or only that of mediocre quality, what should be done? Shall the engineer adopt the process of Mr. Vicat, which consists in making an artificial hydraulic lime? I answer, emphatically, that I think not; in this case, occurring very often, it is, in my opinion, preferable to make hydraulic mortar by a more direct process which I shall point out.

There are two modes of obtaining hydraulic mortar; the first consists in mixing natural, or artificial, hydraulic lime with sand; the second consists in mixing ordinary fat lime with certain substances such as puzzalona, trass, certain coal-ashes, and brick dust, or tile dust. I feel bound to correct here, an assertion touching these mortars, not perfectly accurate, of Mr. Gauthey, Inspector of roads and bridges. In his excellent *Treatise on the construction of bridges*, this Engineer says (Vol. II., page 278) that "fat lime is very proper for constructions out of water; but will not answer in the composition of betons to be placed in water, because the mortars in which it is used, even when mixed with puzzalona, and placed in water as soon as made, do not harden, but remain pulverulent." This is far from exact: because mortar composed of fat lime and puzzalona hardens very soon in water, and acquires, in a short time, very great strength. This fact was known to the ancients, for Vitruvius speaks of it, as will be seen further on.

I should not refer to the error into which Mr. Gauthey has fallen in this instance, if he did not enjoy a reputation so justly elevated. His highly esteemed work being in the hands of every Engineer, it was to be feared that this remark of his would prevent constructors from making hydraulic mortars by the direct union of common lime and substances analogous to puzzalona. My experiments will show that, in countries where hydraulic lime is not to be had, instead of following the process of Mr. Vicat, it is preferable to make hydraulic mortar by a direct mixture of fat lime with substances of a similar nature to puzzalona. These experiments show also that fat lime is far from being always proper for construction out of water; although Mr. Gauthey, in the beginning of the sentence, states it to be.

Art. II. *On slaking Lime; manner of making Mortar; observations on Hydrate of Lime.*

There are three modes of slaking lime. The first consists in throwing on the lime, as it comes from the kiln, enough water to reduce it to thin paste. This process is the one generally employed with fat lime. Too much water is added, almost always—that is to say, as much as is required to make it a thin cream. In this state it is run into vats; after some time it thickens, and it is then covered with a layer of sand or earth to preserve it from contact of the air, which would soon convert the upper portion into a carbonate. It is a common opinion that the longer the lime has been kept in this state, the better it is. My experiments will show that this is not true, at least not always true: since some fat lime that I had experimented with, which had been lying in this condition, gave, in the air, when the mortar was composed of lime and sand only, very bad results.

The thickening of the lime in the vats is due to the escape of water by filtration, by evaporation, and also to a third cause: for this thickening which is quite prompt, occurs equally when the vats are constructed in moist ground, and when the season is rainy. This third cause appears to me to be this: that the lime, having a strong affinity for water, solidifies the first portions very promptly, but requires a considerable time to saturate itself completely. These portions of the lime which have been too much or too little burned are, besides, slow to slake. I made the following experiment to satisfy myself on this point. I took a portion of lime that had been lying wet in a vat for four years, it was quite thick, I added a little water to bring it to the consistence of sirup, and placed it in a stoneware vessel. I took an equal portion of fat lime, slaked fresh from the kiln, reducing this also to the consistence of sirup, and placing it in a similar vessel. After a short time, this last had become very thick, while the former retained its consistence of sirup; I then added water to restore the consistence first given. The thickening again occurred, but more slowly than at first. It was necessary to add water several times before the second lime would maintain the sirupy state. It results from this experiment that fat lime, slaked into a clear paste as it comes from the kiln, retains the power of absorbing water for a considerable time.

The second method of slaking consists in plunging quick-lime into water for a few seconds. It is withdrawn before the commencement of ebullition; slakes with the water it has absorbed, and falls to powder. It is preserved in a dry place. The operation is performed with baskets into which the lime, broken to the size of an egg, is put. Mr. de Lafaye, in 1777, proposed this mode of slaking lime, as a secret recovered from the Romans; it made much noise at the time, but experience has not realized the great results anticipated.

The third process consists in leaving the quick-lime exposed to the air. Its strong affinity for water causes it to attract the greater part of that which is in the surrounding air. Lime, thus exposed, slakes slowly without giving out much heat, and falls at last to powder. This mode of slaking is called air-slaking, or spontaneous slaking. It is employed, more or less, in several countries. It is spoken of in several works on constructions, and is generally condemned. Mr. Vicat, however, appears to give it the preference, for, at page 20 of his memoir, he says: "Such are the three modes of slaking lime: the first is generally used; the second has hardly been tried, except as an experiment at certain works; the third is proscribed, and represented, in all the treatises on construction, as depriving the lime of all energy, to such a degree that those portions which have fallen to powder in the air, are considered as lost. We shall not now speak of the processes of Rondelet, Fleuret, and others, because they do not differ much from those described. We shall see, further on, that, as regards spontaneous slaking, these proscriptions of authors who, believing every thing, repeat without examination the errors of those who preceded them, are founded on false observations and are deserving only of mistrust." Mr. Vicat has announced that a mortar made of sand, and fat lime which was air-slaked, resisted perfectly at the end of ten years, the test indicated by Mr. Berard for frost-proof stones; he says on this subject "a hint, this, to those who have written and spoken so much against air-slaking, and in opposition to the opinion which I have had to maintain singly, unable to invoke to my aid any experiments but my own." The results I have obtained are far from confirming what Vicat says, as will appear by the experiments reported hereafter.

Mr. Vicat gives, at page 20, experiments made by him to ascertain the amount of swelling of fat limes and of hydraulic limes, on slaking by the three modes. He found the first mode to be that which gave the greatest volume of paste, with both kinds of lime. On comparing the bulks obtained by the second and third modes, it was found that with fat lime, air-slaking gave greater bulk than slaking by immersion, and that it was the reverse with hyraulic lime.

At Strasburg, an attempt was made to apply, on a large scale, the mode of slaking by immersion, pointed out by Mr. Lafaye; but the process was found to be attended with inconvenience and embarrassment. It is necessary to procure a stock of baskets—to break up the larger pieces—to secure workmen who will be faithful in holding the lime under water only the given number of seconds, which is not easy; a portion of the lime is lost, falling in powder to the bottom of the vessel of water; when the lime is reduced to powder, it is requisite to measure it before making the mortar, and should there be a wind, much will be lost. These objections caused the process to be renounced in favour of that about to be described, and which amounts to the same thing.

It is founded on the following observation: if quick-lime be plunged into water it absorbs, in a certain number of seconds, a quantity sufficient to reduce it well to powder. We shall have then a like result by throwing the same quantity of water on the lime, and avoid the inconveniences attending the plunging into water. Since 1817, this process has been employed at Strasburg, where considerable masses of lime were operated on. A small building was erected near the works, into which the hydraulic lime, not allowed to arrive too fast from the kiln, was put, to be protected from the weather; the building was boarded on the sides and top, and, in case of rain, covered with a tarpaulin. By the side of this lime-house, a larger shed was constructed, the top only being boarded; a plank floor, on which the mortar was mixed, was laid under this shed. There was a measure, without a bottom, which contained about 10 cubic feet, each dimension of the box being about 2.20 feet, this was placed on the floor and filled with lime; which being done, the same measure was used for the sand, which was placed around the lime, without covering it: with large tin watering pots of known capacity, water, equal in bulk to about one-quarter the bulk of the lime, was thrown on: the workmen knew they were to empty the watering pots but a given number of times; and the lime being all in sight they saw that they should throw the greater quantities on those parts of the heap where lay the largest lumps of lime. As soon as the slaking became energetic, the lime was left to itself until the vapours had ceased; it was then turned a little with a shovel, or a rod was thrust in, and if any lumps were found still entire, either for the want of water, or because they were too much burned, a little water was poured on these lumps. A regular form was then given to the heap, and the surface being slightly pressed with the back of the shovel, the lime was covered with the sand that had been placed around it. This process was completed towards evening—as many heaps being prepared as it was presumed would be required during the whole of the ensuing day. By thus leaving the lime, over night, in heaps, the slaking is complete; portions which have too much water impart it to those which have too little, and the water becomes thus uniformly diffused through the heap. In the morning the sand and lime of each heap were mixed together, and passed twice under the rab (rabot) before adding any water: in this way, if there were any stones, or pieces of lime imperfectly slaked, they were easily found and rejected. Water was then added in sufficient quantity to bring the whole to the state of very soft paste; because in this dilute state the mortar is, with less labour, mixed more perfectly. Experiments which follow will show that it is an error to insist that mortar should be mixed with "*the sweat of the labourers:*" it is enough if the sand be well mixed with the lime; and this mixture is better effected, and in a much more economical manner, when the mortar is in a state rather thin, than when it is thick; another reason for making it rather thin is, that it often becomes stiffer than it ought to be, before it is used, in consequence of the lime preserving, as before stated, for a considerable time, the property of solidifying water. When the lime has been properly burned, the operation just described gives a homogeneous mortar not at all granular, and not exhibiting a multitude of little white specks, which are particles of lime that have been badly slaked. At Strasburg the precaution was always taken of making up only one or two heaps of mortar at a time; so that it should not have too much time to dry before being used, and that the masons might find it in the state of paste, in the heaps in which it was deposited after being well worked. In making the mortar only as it is needed, there is, besides, the advantage of avoiding the labour of remixing, in the frequent case of the works being interrupted by rain: it is best therefore to make the heaps of slaked lime into mortar, no faster than as they are wanted. Lime may be preserved in this way for eight or ten days without losing quality. If at the end of the day, all the heaps of lime have been consumed, new heaps should be formed for the consumption of the morrow: if a portion only have been consumed, this portion should be made good. This manner of slaking lime, and making mortar, gave very good results at Strasburg, and at other places in the vicinity where it was employed. It is seen to be a method analogous to that by immersion, recommended by Mr. Lafaye; but, by throwing upon the lime, just the quantity of water necessary to reduce it well to powder, instead of immersing it, much inconvenience and embarrassment are avoided, especially when operating with large quantities of lime.

An opportunity was presented of convincing ourselves of the goodness of mortar made by this method, it being necessary, in order to make a postern, to pierce through a revetment wall that had been built two years: the mortar had already acquired such hardness that the tools had much difficulty in cutting through the masonry.

In extensive works, it will be very advantageous, as regards economy, to make mortar with a machine. Several have been contrived with this object, but that which has succeeded best is a two-horse machine proposed and executed by Mr. Saint-Léger, formerly Captain of Engineers. The following is a brief description.

A circular trench, having the two sides sloping, is built of masonry; the section of the trench is a trapezoid 2 feet wide at bottom, 3 feet 4 inches wide at top, and 1 foot 4 inches deep; the inner circle of the trench is 9 feet 4 inches in diameter; at the centre there is a mass of masonry, in which is fixed a vertical axis, of wood, 6 feet 8 inches long, and 8 inches square, and which is bedded in the masonry about 5 feet; the top of this axis is formed into a cylinder 5 $\frac{2}{10}$ inches in diameter, and 6 inches high; around which is fitted a collar of cast-iron, carrying laterally two horizontal trunions 3 $\frac{2}{10}$ inches in diameter, and 4 $\frac{8}{10}$ inches long; a piece of wood, 26 feet 8 inches long, is notched at its middle upon the collar of the vertical axis. (Instead of one piece of wood, two might be taken, each 13 feet 4 inches long, by strongly securing, with iron, their junction with the vertical axis.) This piece is placed horizontally, and is about 13 inches square in the middle, lessening towards the ends, so as to serve as an axletree to two vertical wheels with broad felloes—6 feet diameter of wheel, and 6 inches breadth of felloe. These two wheels rest in the circular trench in such a way that the one touches the exterior and the other the interior slope of the trench. A horse is attached to each extremity of the horizontal bar, and their united efforts cause the wheels to revolve in the trench; behind each wheel, attached to the horizontal bar, by means of a hinge, is a scraper of wood armed with iron, these follow the movement of the wheels, scraping the two sides of the trench so as to throw the mortar under the wheels. These scrapers of which the lower end is within two inches of the bottom of the trench, are attached by hinges in order that they may rise over any obstacle.

Mortar is made in this machine in the following manner. A cubic metre (35.34 cubic feet) of lime in the state of paste is thrown into the trench, and the horses are started; a little water is added if necessary, and when the paste has become quite liquid and homogenous, the proper quantity of sand is thrown in by the shovel, without arresting the movement; in about 20 or 25 minutes the mortar is made. With this machine 12 batches of 3 cubic metres each (12 × 3 × 35.34 equals 1272:24 cubic feet) may be made in 10 hours labour; the reqisite agents being 4 labourers, 2 horses and their driver, and 1 superintending mason.

The expense of making a cubic metre of mortar, amounts in Paris to about $0.10; this is a considerable saving over the common mode of making mortar.* It is desirable therefore that frequent use be made of this machine, in places where there are important constructions. The description just given is extracted from the *devis-modèle* of the corps of Engineers, and was prepared by Lt. Col. Bergère of the Engineers.

It is stated above that at Strasburg, lime which was to be made into mortar, was slaked to dry powder, and left in that state for twelve hours at least before giving it the quantity of water necessary to convert it into paste. I made the following experiments with limes of the environs of Strasburg, to ascertain the volume obtained in powder and in paste, when the proper quantities of water are used to produce those states.

TABLE I.

Designation of the lime of which the volume is taken as unity.	Volume of water used to bring it first, to a state of dry powder.	Volume produced of dry powder.	Volume of water used, in all, to bring to state of paste.	Volume produced in state of paste.
Lime of white marble	$\frac{1}{2}$	$2\frac{1}{2}$	$1\frac{6}{10}$	$1\frac{1}{2}$
Fat lime of Strasburg	$\frac{1}{2}$	$3\frac{1}{2}$	2	$1\frac{3}{4}$
Yellow lime of Obernai	$\frac{1}{2}$	2	$\frac{3}{4}$	1
Blue do. do.	$\frac{1}{2}$	$2\frac{1}{3}$	$1\frac{1}{6}$	$1\frac{1}{6}$
Brunstat lime	$\frac{1}{2}$	$2\frac{1}{2}$	$1\frac{1}{2}$	$1\frac{1}{2}$
Villé lime	$\frac{1}{2}$	$3\frac{1}{3}$	$1\frac{1}{2}$	$1\frac{1}{2}$
Altkirch lime	$\frac{1}{2}$	$1\frac{5}{6}$	$\frac{5}{6}$	1
Verdt lime	$\frac{1}{2}$	$2\frac{1}{4}$	1	1
Metz lime	$\frac{1}{2}$	$2\frac{1}{3}$	$1\frac{1}{6}$	$1\frac{1}{20}$
Boulogne pebbles	$\frac{1}{3}$	$1\frac{1}{6}$	$\frac{1}{2}$	$\frac{3}{4}$

All the limes in the above table, were used fresh from the kiln. I reduced them to powder in a mortar, sifted them, and used, for quantity, about one quart. Thus, for example, I took a measure of quick lime of white marble, and throwing upon it half a measure of water, I obtained 2½ measures of lime slaked to powder, which I measured after it was cold. The quantity of water thrown on is shown in the second column, and the quantity of lime obtained in powder is given in the third column. I was obliged to throw upon this lime in powder, one measure and one-tenth of water in addition to reduce it to paste. Adding this last quantity of water

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* One day of superintending mason, ...	$0 66½
Four days of labourers, at $0 28½, ...	1 14
Two days of horses, at $0 42¾, ...	0 85½
One day of driver, ...	0 38
Wear and tear and repair for one day, . . .	0 09½
Greasing wheels one day, ...	0 07
Wear and tear of shovels and barrows, 1 day for 4 labourers, .	0 07½
Contingencies, 1-10th of all, . . .	0 32
Total expense for 36 cubic metres = 1272 cubic feet	$3 60

Equal to $0 10 per cub. metre—or $0:0028 per cubic foot.

The quality of the mortar is superior to that made by the common process; and it is well to remark, that the time during which the mortar is made is precisely that in which the labourers repose: it is therefore their interest to let the machine go as long as possible, and consequently to render the mortar more perfect, so that the supervision will be directed chiefly to the proportions of the mixture. This note is extracted from the devis-modèle du corps du Génie, p. 71.—Tr.

to the half measure used in the first instance, the total is 1 $\frac{6}{10}$ measures of water, absorbed by the lime, in being reduced to paste: this is shown in the fourth column. The fifth column shows that I obtained 1½ measure of lime in paste. I followed the same process for all the limes of the above table, producing a uniform consistence of paste, by adding the water little by little. Experience had taught me that these limes were reduced to dry powder by throwing on one-fifth of their bulk of water; and that as much as one-half their bulk might be thrown on without the powders ceasing to be dry: beyond this term, a moist powder would be obtained. The only lime on which I threw less than half its bulk, was that at the bottom of the table, of the Boulogne pebbles; on this I poured but ⅓ its bulk of water; as this lime forms a moist powder with ½ its bulk of water, I was obliged to restrict myself to one-third. This table shows that these different limes afforded very different volumes of powder with the same quantity of water: that the quantities of water absorbed to produce the state of paste were very different, and, also, that the volumes of paste differed much. Experiments which follow will show that, of the limes in the table, those are the most hydraulic which absorbed the least water in passing to the state of paste, and which gave the smallest bulk both of powder and of paste. Those limes, of the table, which are not hydraulic, are those which gave the greatest volumes in powder and in paste. There are in the table two kinds of Obernai lime, one yellow and the other blue; they are of the same limestone, but one more highly calcined than the other. When this lime has been burned just enough, it is of a yellow-fawn colour; when a little more burned, it is of an ashy-gray, and when too much calcined, of a decided blue. It was upon the two extremes of calcination that I made the above experiments, they show that the degree of calcination has a sensible influence on the swelling of this hydraulic lime.

As the swelling of lime, shown in the above table, was obtained with quite small quantities, and with pulverized quicklime, I caused experiments to be made at the mortar beds on a large scale, with fat lime and with Obernai lime; these being the two kinds of lime ordinarily used upon the works. The following results were obtained. Fat lime was taken immediately from the kiln, and measured in the boxes in use at the mortar beds; care being taken to break up a portion of the lumps of quick lime into smaller pieces, in order to occupy the interstices between the larger pieces, and to have the measure well filled: water, in quantity sufficient to bring the lime at once to paste of the consistence of mortar, was thrown on without delay, and the quantity of paste thus obtained was measured. Proceeding thus—one measure of quicklime, just from the kiln, required two measures of water to produce the state of paste, and yielded 1.83 of paste, which differs but little from Table No. 1, wherein the produce is 1.75. The same operation was repeated with Obernai lime, after having rejected vitrified pieces, and those which had not been sufficiently calcined: one measure of this lime absorbed 1.30 of water in being reduced to paste, and in this state gave 1.30 of lime. This differs somewhat from the result in the Table.

The difference may be owing to this, that in the experiments of the Table, the lime was pulverized, and was twice slaked; that is to say had two successive applications of water, while in the larger experiment the lime was not broken up, and had water poured on but once. The degree of calcination might, also, have had some influence.

Many metallic oxides are susceptible of absorbing and solidifying a certain quantity of water forming compounds which possess peculiar properties.

It is to these compounds that the term hydrate has been assigned. It has been seen, above, that lime is a metallic oxide, and that this substance absorbs and solidifies a large quantity of water; but the quantity of water absorbed by lime in forming its hydrate is not exactly known. Berzelius asserts that the hydrates are formed of water and oxides in such proportions that the quantity of oxygen contained in the oxide is equal to the quantity of oxygen contained in the water; but Mr. Thenard does not admit this law: he says that the experiments on which it is founded are not numerous enough, nor sufficiently precise, to allow its definitive admission. It is certain nevertheless, says this celebrated chemist, that amongst the hydrates which have as yet been examined, those which contain the most water, are those, also, of which the oxides contain the most oxygen. According to Berzelius, the hydrate of lime is obtained by throwing upon quick lime the water necessary to reduce it to thin paste (*bouille,*) and exposing this paste in a silver or platina crucible to the heat of a spirit-of-wine lamp. After having dried the hydrate of lime in this manner, it is weighed, and the quantity of water it has absorbed is known by the augmentation of weight. Berzelius made two experiments, one with 10 grammes of lime and the other with 30 grammes. He found in the first experiment, that the lime had increased in weight 32.1 per cent., and in the second, 32.5: in this second experiment there was, therefore, an augmentation of four-tenths more than in the first. He attributes this difference to an absorption of carbonic acid, and he admits, as good, only the first experiment, in which 100 parts of pure lime containing 28.16 parts of oxygen, are combined with 32.1 parts of water containing 28.3 parts of oxygen; whence Berzelius concludes that the water absorbed by pure lime contains a quantity of oxygen equal to that contained in the lime.

I have repeated the experiment of Berzelius by operating on 20 grammes of pure lime, using, as he did, a spirit-of-wine lamp, and a platina crucible. I was surprised at obtaining an augmentation of only 22.5 per cent. I repeated the experiment several times, successively diminishing the thickness of the wick, and as I did this, the lime retained more and more water. I inferred, therefore, that the hydrate of lime decomposes with a feeble heat; and that, if Berzelius obtained a greater result in the second experiment than in the first, it was not all due to the absorption of carbonic acid, seeing that the operation lasts only a short time; but to this, that heating with an equal flame, two volumes of hydrate of lime, of which one was triple the other, the smaller volume should lose most water by the heat. But there is a fact which proves with how great facility the hydrate of lime abandons a part of its water. All those who have made mortar of lime newly slaked, have perceived that it becomes very dry in a short time. If, when in this state, it be worked for some time without adding water, it will be brought back nearly to the same moist state it had at first; and drops of water may be seen on the mortar. The same result is obtained with lime alone. It follows from this, that simple friction (working) decomposes the hydrate of lime, and that a feeble heat produces the same effect. To know, therefore, the quantity of water which enters into the hydrate of lime, it appears to me that other means of drying should be resorted to than fire.

The various kinds of lime are used in constructions, only after having been brought to the condition of hydrate: nothing, therefore, that relates to the properties of this compound, is a matter of indifference. As yet, few experiments have been made to determine the quantity of water that should be given to lime in making mortar. I proposed undertaking several experiments on this point, but time failed me. The matter should be attended to, because, opinions are much divided thereon, for want of exact experiments.

The following are the principal properties of hydrate of lime: it is white, pulverulent, and much less caustic than quick lime; it easily abandons to heat the first portion of water, but it requires a high temperature to drive off all the water entering into its composition. This hydrate absorbs carbonic acid; experiments which follow show that it has, also, the property of absorbing oxygen, and that lime sustains important modifications in consequence of this absorption of oxygen. According to the chemists, lime is incapable of absorbing a fresh quantity of oxygen: but according to my observations, there is no doubt that the hydrate of lime absorbs a considerable quantity. I shall give, in the following Article, experiments which I made on this subject.

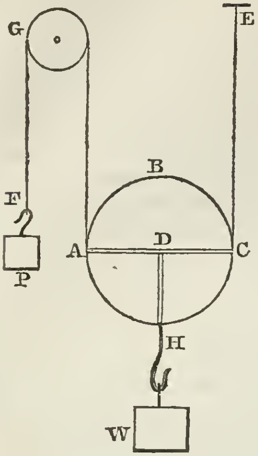
[TO BE CONTINUED.]

Mechanical power of the Pulley.

COMMUNICATED FOR THE JOURNAL OF THE FRANKLIN INSTITUTE. BY L. H. PARSONS

In all works on Mechanical Philosophy that I have seen, the *Pulley* is treated of as a distinct principle, so far as respects the mechanical advantage, or power, which it affords. Its efficacy is supposed to be owing, some how, to the peculiar combination of the *cords* that are used. It is sometimes indeed denominated the "*funicular* system." That a wheel or pulley, revolving on an axis, is a *lever*, in the same sense in which a common scale-beam is a lever, I suppose is never denied. But, as a lever of that description, that is, of the first kind, with the fulcrum at the centre, admits of no mechanical gain,—the velocity of the weight being equal to that of the power,—that view is obviously inadequate to the explanation of the *movable* pulley. Either the fulcrum is not at the centre, or the movable pulley does not act upon the principle of the lever. The latter conclusion is adopted. No writer, that I am aware, attempts to explain the difference in the velocity between the power and the weight, on that principle. In the Library of Useful Knowledge, it is expressly asserted (I do not quote the language) that the pulley does *not* involve the lever principle. The explanation universally given, is, that the weight being supported by the several cords attached to the movable pulley, or pulleys, the resistance is distributed equally among them; and consequently, the power being applied to only one of the cords, it encounters only that proportion of the resistance, which unity bears to the number in use.

However clear this exposition may appear to others, to me it was never satisfactory. That the resistance of a weight, supported by two or more cords of equal tension, is *divided* between those cords, is, to be sure, quite clear. The same fact, however, would be true, if the several cords were fastened directly and separately, to the weight, instead of passing over pulleys. But, (it may be answered) if all the cords were fastened to the weight no one could be *moved* in the direction of the power, without instantly encountering the whole resistance, and consequently communicating a motion to the weight, equal to that of the power. True: and why is not that the case with a pulley? Why does the power, as well as the cord to which it is attached, pass over twice as much space, as the weight passes over? Not, I apprehend, because the resistance is divided between the several cords. The two facts co-exist, in the case of a movable pulley; but the one is not a consequence of, and cannot be demonstrated by, the other.



It appears to me clear, and if I mistake not, it is perfectly demonstrable, that the movable pulley *is a lever*—not of the first, but of the second kind ;—with the weight between the power and the fulcrum. By considering the fulcrum at the *end* of the lever, instead of the centre,—that is, at the *side* of the pulley, where the stationary cord comes in contact with it, the difficulty disappears. The accompanying diagram, with a very few words, I think will make the matter plain. A B C is a movable pulley. The power P is obviously exerted at the point A. It must be equally obvious that the weight W rests upon the *centre* D of the pulley A B C, or of the lever A C; while the point C, of the *stationary* cord E C, serves as a fulcrum, or *centre* of *motion* to the whole pulley. C being the real centre of motion, and A being a point in the radius A C, at twice the distance of D from the centre, it follows that the velocity of A, must be double that of D.

If any doubt should remain in relation to this reasoning, let it be imagined that the *pulley* is all removed, except the *bar* A C ; the other parts remaining the same as before. Would the removal of the pulley, affect the *principle* of the machine at all? Manifestly it would be the same machine, except that a circular wheel is, for such a purpose, a more perfect lever, than a straight bar; inasmuch as the bar would soon lose its horizontal position, and consequently would often need readjusting: whereas the wheel is a self adjusting lever.

It will be obvious, I think, from these remarks, that the power or efficacy of the pulley, does not reside peculiarly, in the *cords*, as is, perhaps, generally believed. The cords are, practically, an indispensable appendage; but theoretically they are not an essential element,—at least in demonstrating the principle. Suppose that, not only the pulley, but the cords were removed from the apparatus, represented above, and the point C rested on a solid abutment, instead of being supported by the cord E C; and suppose the power were applied immediately at A, would not the principle remain precisely the same?

There is one objection, which it may be proper to notice. It is said, and with truth, that even the pulley itself is not an essential part of the machine bearing that name. If, instead of passing over a pulley, the cord should pass through a ring attached to the weight, the same relative velocity between the power and the weight, and the same efficiency would exist,—setting aside friction, and the rigidity of the cord. How then, it may be asked, can a solid ring or flexible cord, or both together, constitute a lever? In the first place, it is to be observed that the *length* of a lever is of no account, in determining its efficacy. The only essential point in order to give a lever of the kind now under consideration, a power of two to one, is, to have the weight *half-way* between the power and the fulcrum. Now the point where the stationary cord comes in contact with the *ring*, is as truly a fulcrum, as if a pulley, or real lever should be interposed. And the other cord as truly exerts the power at the point where it comes in contact with the other side of the ring.—Nay, these two elements, the power and the fulcrum, would remain, even if the *ring* should be removed and no solid body be between them. It is enough that one point is movable and the other vertically stationary, for the time being. The *loop* forms the connexion between those two points;—and passing over a ring, or other solid body, it creates a fixed and determinate relation between them and the weight, as effectually as is done by an inflexible rod.

We insert the foregoing communication, although the writer appears to labour under some misapprehensions with respect to the nature of the pulley. His views are not so clearly expressed as to leave us, in all places, perfectly free from doubt with respect to his meaning.

Several authors have explained the relation between the power and the weight in the single movable pulley on the principle of a lever of the 2nd kind: others, however, have properly objected to this mode of treating the subject, on the ground that the cord is the real mechanical power, and that the only advantage of the pulley consists in reducing the effect of friction and obviating the imperfect flexibility of the rope. Suppose the pulley to be removed and the weight attached to an infinitely fine wire crossing the rope in a direction perpendicular to the plane of its two branches, let the rope be pefectly flexible and move without friction and we have an imaginary machine in which there is the same relation between the power and weight as in the movable pulley: but here there is no lever, since the points before considered as the points of application of the power and weight respectively, now coincide, and of course move through equal distances in equal times; hence if the lever can still be supposed to exist, the power and weight having equal motions must be themselves equal, but this is contrary to the fact. If then the pulley is to be considered a lever, the cord must be a separate mechanical power; but the principle of the cord being applicable to all the varieties and combinations of the pulley, it is better to consider the pulley, as it is in fact, a mere practical convenience in the use of the cord, and theoretically in no way affecting its efficacy.

The writer of the article before us seems to consider it necessary to introduce the principle of the lever to prove that in the case of a single movable pulley the power should move through twice the distance of the weight—the fact is however clearly demonstrable in a very simple manner.

Let the power P descend 12 inches; then the branch F G will be lengthened 12 inches, and consequently the length of G H E will be diminished by the same amount; but as the weight must always take the lowest position possible, G H must always be equal to H E; hence each will be shortened 6 inches; and through this distance therefore the weight will rise.

G.

Observations on Mr. Beard's communication on the subject of belting as a substitute for gearing by wheels. By Mr. Rufus Tyler, U. S. Mint.

FOR THE JOURNAL OF THE FRANKLIN INSTITUTE.

TO THE COMMITTEE ON PUBLICATIONS.

Gentlemen,—I noticed with much satisfaction the valuable communication of I. H. Beard, Esq. on the subject of Mill Gearing, which appeared in the June number of your Journal.

I have long had a desire to see an exposition of that subject, by some one thoroughly acquainted (as the writer of that article appears to be) with the belting system as practised in the eastern cotton and woolen manufactories.

On examination of the article alluded to, I was impressed with the belief that some additional benefit might arise from a more minute investigation of several of the points there introduced, to aid in satisfying the minds of such of your readers as do not readily arrive at the rationale of mechanical operations and arrangements, especially as some of the statements appear somewhat embarrassing and likely to lead to scepticism as to the correctness of his conclusions.

I entirely agree with Mr. Beard in regard to the many advantages of the belting system over that of wheel gearing, and that "to gear a mill wholly with belts, and to do it judiciously, and to the best advantage, doubtless requires more nice calculation, careful judgment and practical experience, than to do it with gear wheels." "For many mills have been so belted as to cause more friction, trouble and expense, than would be caused or required in the use of gear-wheels."

After giving, as objectionable, one of the worst methods of arranging the main belt that ever was devised, (to wit, at page 453 in which no less than *ten* lines of drums, requiring four binders, to strain and give direction to the belt, are represented as being propelled by a single belt of great length,) he says, "in passing the drums the whole stress upon the belt is thrown upon the journals of the shaft of each drum the belt passes, which besides greatly increasing the power required to operate the mill, on account of the multiplied friction, causes the journals to heat and wear, beyond the power of any lubrication to prevent."

This passage though sufficiently explicit to convey the general idea intended by the author, leaves room for inquiry as to the relative degrees of force upon the *belt* and *journal* as well as the comparative intensities or degrees of stress upon the *different parts of the belt.*

If we raise a weight of 100 pounds by a cord or strap passing over a series of drums, (running on and off in parallel lines) the first drum in the series, or that nearest the weight, will be pressed against the bearings of the journals by a force of 200 pounds, besides the additional force upon the strap at one side of the drum, required to overcome the friction of the drum. For the strap is pulled from the drum on one side, by the weight, and on the other by an equal force required to overcome the weight, both acting in the same direction, and consequently making *one* force equal to their *sum,* which reacts upon the drum, and the additional force necessary to produce motion also re-acting in the same direction.

The tension of the strap thus increased, after passing the first drum in the series, will be thrown in *double force* upon the journals of the second, and then require a *further increase of strain* to overcome the *friction* of the *second* drum, and so on, each succeeding drum being subject to an increase of pressure upon the journals to cause friction, and is attended with an increased stress upon the strap which passes over it; in other words, each drum the strap passes, is pressed against the journals by a force equal to twice the tension of the strap, before it reaches the drum, added to the force necessary to overcome the resistance of the drum itself.

Now if there be ten drums in the series, and we suppose each one to be required to raise a weight of 100 lbs., it is evident that a much higher rate of increase will prevail in the tension of the strap in passing the succeeding drums.

Indeed, leaving out the friction altogether, the *strain,* upon the strap in passing the last drum would be ten times as great as at the first, (i. e.) 1000 pounds, and the pressure upon the journals double that intensity.

To carry out this illustration in the case of an actual belt, driving a series of drums, as represented by Mr. Beard, we may substitute (in place of weights representing the resistance to be overcome) a friction clasp or brake, at the journals of each drum, which shall be equivalent to 100 pounds resistance at the periphery of the drum, and acting in such a manner as to avoid pressing the journals against the bearings to cause friction, and let this resistance represent the ordinary operations of the mill, of which each drum performs an equal share. It is evident that after passing the first drum, the belt must be strained to the tension of 100 pounds, and on the other side only just enough to cause the necessary adhesion of the belt upon the drum, (say 50 pounds) making with the 100 pounds upon the pulling side, the sum of 150 pounds pressure on the bearings of the journals of the first drum to create friction.

Omitting for the present the friction of the journals, we shall find that on passing the second drum, the belt must be strained with 100 pounds more force, or 200 pounds upon the pulling side, which with the 100 acting to move the first drum, makes the pressure on the journals 300 pounds. We have now a tension on the belt of 200 pounds to be carried around the third drum creating a pressure of 400 pounds, to which we must add 100, arising from the increased tension of the belt requisite to make this drum revolve; making the pressure against the bearings of the third drum = 500 lbs., the tension of the belt being now 300 pounds. Proceeding in this way the pressure on the bearings of the fourth drum will be 300 + 300 + 100 = 700 pounds, the tension of the belt 300 + 100 = 400. The pressure on the fifth drum 400 + 400 + 100 = 900, tension 500; pressure on the sixth drum 500 + 500 + 100 = 1100, tension 600; pressure on the seventh drum 600 + 600 + 100 = 1300, tension 700; pressure on the eighth drum, 700 + 700 + 100 = 1500, tension 800; pressure on the ninth drum 800 + 800 + 100 = 1700, tension 900; pressure on the tenth drum 900 + 900 + 100 = 1900; tension of the belt 1000 pounds.

This accumulated stress upon the belt must next be thrown upon the *main or driving drum*, which with the necessary strain upon the other side of the main drum to cause the requisite adhesion of the belt (say 50 per cent. as at first assumed = 500 pounds, will give as the pressure upon the journals of the main drum the sum of 1500 pounds. But as the belt goes with its present strain, 500 pounds, to the next succeeding drum, which brings us round to the one we started with, where only 50 pounds is required as the tension of the belt, (and which, under a simple arrangement, would be sufficient) we have no alternative, but must add the surplus of 450 pounds to that assumed in the beginning, which taken twice makes 900 pounds *to be added* to the amount before given as the pressure upon the journals of each drum, except the main or propelling drum. Thus for the

First,	instead of	150	say	1050
Second,	"	300	"	1200
Third,	"	500	"	1400
Fourth,	"	700	"	1600
Fifth,	"	900	"	1800
Sixth,	"	1100	"	2000
Seventh,	"	1300	"	2200
Eighth,	"	1500	"	2400
Ninth,	"	1700	"	2600
Tenth,	"	1900	"	2800
Total amount of friction		10050		19050

From what has been shown, we learn how vastly more important it is to avoid all unnecessary resistance at the drums which are remote in the train, than it is with drums situated near the propelling power.

It will be perceived likewise, that if the several drums before mentioned had each a separate belt direct from the main drum, none of them would have more than 150 pounds pressure upon the journals, and therefore that the aggregate amount would be only 1500 instead of 19050 pounds; and were it practicable to carry out the plan of throwing the strain of one belt into that of another as is sometimes done in particular instances, and as is recommended by Mr. Beard (page 455) in the communication before us, the whole amount of pressure, upon the journals of all the drums, would be (theoretically) but the bare 150 pounds, and *that* upon the furthest in the series from the main drum, exclusive of what is on the main drum.

It now remains to show the effect of the friction which arises from the tension of the belt, and to show the whole amount of power *wasted*, compared with that required to do the work.

The *force* of friction at the rubbing surface may be taken at one-seventh of the pressure. Now the first drum of the series, in the foregoing example, was found to be loaded with a pressure upon the journals of 1050 pounds (exclusive of any effect of the friction caused thereby in adding to the tension of the belt and consequently increasing the pressure) one-seventh of which is 150 pounds at the surface of the journals.

Allow the drum to be ten times the diameter of the journals, and the force of resistance at its periphery will be $\frac{1}{10}$th of the intensity, or 15 pounds $= \frac{1}{70}$th of the *pressure*, the degree in which the stress of the belt must be *increased* to meet the demand of *friction.*

This increased tension of the belt, extends to the main drum, re-acting upon the first only 15 pounds, twice that force, or 30 pounds upon each of the remaining 9 drums over which it passes. The sum of these pressures $30 \times 9 + 15, = 285$ pounds the pressure due to the friction of the journals of the first drum.

The pressure found for the second drum, as given in the table 1200, and the above 30 pounds from the friction of the first drum, is 1230 pounds for the actual pressure against the bearings of the second; $\frac{1}{70}$th of this pressure, or 17 $\frac{4}{7}$ths pounds goes to increase still further the tension of the belt beyond this second drum, throwing 17$\frac{4}{7}$ths pounds additional pressure on the bearings on the second, and twice that force upon each of the remaining eight, or, omitting fractions, $35 \times 8 + 17 = 297$ pound pressure due to the friction of the second drum.

Again $\frac{1}{70}$th of the 1435 pounds pressure on the third drum, as the increased tension of the belt, added to the pressure of the third, and twice upon each of the seven remaining drums, gives $40 \times 7 + 20 = 300$ pounds for the friction of the third drum; fourth drum 300; fifth drum 286; sixth drum 261; seventh drum 224; eighth drum 175; ninth drum 114; tenth drum 41 pounds, in whole numbers, as the pressures on these several drums, from these several degrees of friction arising out of their respective proportion of labour and relative position in the series.

The sum of all these pressures is 2226 pounds, which added to the sum of the pressures before set down as due to the proper labour of the several drums, amounting to 19050 gives 21276. If to this we add 1500 pounds, the pressure before given upon the journals of the *main drum*, the amount will be 22776 pounds, as the entire pressure (sufficiently accurate for our purpose) upon all the drums upon the plan and under the conditions specified. Whereas, upon the plan of having a *separate belt* for every drum, leading from one side of the main drum, allowing 150 pounds for each drum, and the same for each upon the main drum, that is 300 pounds pressure for each of the ten drums, and 64 which is $\frac{1}{7}$th of the whole, on account of the friction arising therefrom, the total of pressure will be 3064 instead of the 22776 pounds, whilst the sum of the tension of all the belts, at the tightest side, will only equal *half the greatest tension of the long belt* employed in the foregoing plan.

The effect of friction, taken at the surface of the drum (where the labour to be performed, was estimated,) say $\frac{1}{70}$th of the pressure, will be, to consume $\frac{308}{1000}$ths as much power, as is required for the proper labour of the mill, in the one case, and only $\frac{44}{1000}$ths in the other case—or as 1 to 7.

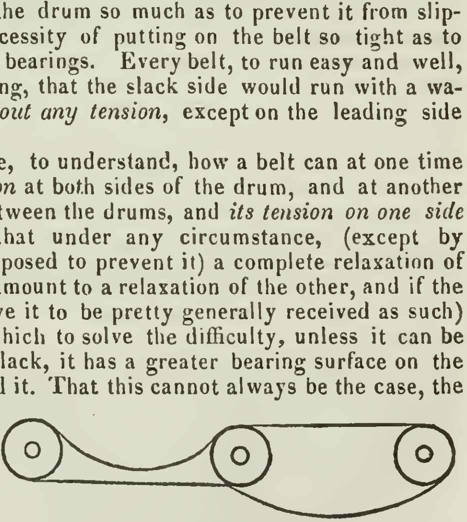
After some appropriate remarks upon the mode of arrangement, which places the line of main drums at one side, instead of in the middle of the room, Mr. Beard gives what he considers the best practicable plan—a plan which, according to my limited knowledge of cotton and woolen mills, appears a very judicious one for accomplishing so desirable an object, as that of the pressure of one belt, counteracting by another, thereby preventing a vast consumption of the power, by unnecessary friction.

A few remarks upon another point advanced by Mr. Beard, will close this notice of the very important subject before us, which I should have been gratified to have seen from an abler hand than mine.

The point alluded to is that of the advantages of placing drums so far asunder, when circumstances will permit, as to give the belt sufficient *length, to cause adhesion to the drum, without too great a tension of the belt.*

"It is," says he, "of great importance, that each belt should be of such a length, that it will adhere to the drum so much as to prevent it from slipping, and that without the necessity of putting on the belt so tight as to cramp the drums and wear the bearings. Every belt, to run easy and well, should be so slack when running, that the slack side would run with a waving undulating motion, *without any tension*, except on the leading side &c."

It will be difficult, I believe, to understand, how a belt can at one time owe its adhesion to its *tension* at both sides of the drum, and at another time, to its *greater length*, between the drums, and *its tension on one side only*. For it should seem that under any circumstance, (except by some adhesive substance interposed to prevent it) a complete relaxation of tension upon one side, would amount to a relaxation of the other, and if the fact be as stated (and I believe it to be pretty generally received as such) I know of no principle upon which to solve the difficulty, unless it can be shown, that when one side is slack, it has a greater bearing surface on the drum, by lapping further round it. That this cannot always be the case, the following cut will show. The cut represents a section of these drums, the middle being the driving one, the belts of course leading off right and left.



It is obvious on inspection, that whilst one of the belts will gain by lapping farther around the drums, the other will loose in that respect.

It appears necessary therefore to seek some other explanation—may it not be owing to the continual flapping of the belt, which increases the tension for the moment in a sort of *twitch*, at each vibration, and that the momentum of the drum, continues it in motion, during the interval when the tension is of course diminished ?

Admitting this *suggestion* to be correct as far as it goes, still there would be an appreciable difference; it would seem, in the two belts repesented in the cut for the reasons before suggested, besides leaving a considerable portion of the strain and pressure to destroy the belt and wear the bearings. One thing is certain that whether the belt be long or short, one means of securing it from slipping is to make it *shorter*.

There is much justice in the remarks of Mr. B. that sufficient care is seldom taken to have belts to run free and easy, and it has been one of the greatest errors more or less prevalent in all Cotton and Woolen Mills to run the belts so as greatly to injure the belts, and rapidly increase the wear of the bearings—and when belts will so run without slipping upon the drums or pulleys, they will wear for a great length of time. For although a belt may be heavily loaded, yet, if at every revolution it can have an opportunity for relief from its tension, so as to contract to its natural texture, it will prevent it from breaking by the stress upon it?

May not the belt of greater length be more durable, chiefly on account of its being less frequently strained, and bent and straightened in passing round the drums or pulleys?

One word on the subject of high velocities: Mr. B. says "a belt adheres much better and is less liable to slip when it runs at a quick speed, than at a slow speed." I have frequently before heard this thing asserted, and have myself observed something of the kind in a case where the work done was interrupted or irregular.

Not being able at this time to assign any better reason, I attributed it to the greater *momentums* of the moving parts, acting in a degree as a fly wheel. In the case alluded to a rope instead of a belt was used.

Yours, &c.

Physical Science.

Experiments on Solar Light. By JOHN W. DRAPER, M. D. Professor of Chemistry, Hampden Sidney College, Va.

(Continued from p. 114.)

115. *Effects of light on vegetation.*

115. Botanical authors have long been aware of the important effects which solar radiations exercise over the colour of vegetables. A plant, which grows in the dark, is of a pale whitish colour, and of a transparent aspect, possessing none of that greenness and vigour which is so characteristically developed on exposure to the sun; its consistency is watery, and although its growth may not be stunted, its appearance is very sickly, its secretory actions are not duly performed, and all its vital operations are carried on in a state of force. There is no longer any evolution of nitrogen from the leaves, and consequently no apparent production of oxygen gas. Light, which seems to act merely as a stimulus on the green organs of vegetables, indirectly bringing about the decomposition of carbonic acid, though accessory is not however essential to the growth of plants. Sub-

teranean cavities, and places far removed from the direct solar ray, have a flora of their own; and in the abysses of the ocean, at depths to which no solar beam can penetrate, and where there is a perpetual night, green plants are found flourishing.

116. The green colour of leaves, is presumed to be an immediate consequence of the act of decomposing carbonic acid. (Decandolle phy. des plantes) It appears to me, that there is some obscurity, if not an actual error, in the view which botanists take of this matter. They suppose, that by the stimulus of light, some portion of the green organ is enabled to decompose that gas, *completely*, or to accomplish its actual resolution into an equivalent volume of oxygen, with the entire deposition of the carbon in the solid form; that it is moreover this carbon, so deposited, that gives origin to the green colour, seeing it forms the *chromule verte* itself. Much useless ingenuity has been thrown away by some chemists in explaining, how carbon, the colour of which is black, or a deep Prussian blue, can produce a lively green, and even if their supposing that the modifying action of a yellow tissue spread over it were correct, of which there is much doubt, considering the thinness of that tissue, and the lightness of its tint, yet certainly we have no necessity to resort to any such explanation. The deposit is not carbon chemically, it contains both oxygen and hydrogen in unknown proportions. Of all the physical characteristics of a body, colour is the most inefficient, it is even proverbial, that after uniting in a new mode, compounds never bear the colours of their constituents; nay more, carbon itself is not essentially of a black colour, as the diamond proves.

117. To a deposit of some compound, in which carbon enters as an ingredient, we are to refer the green colour of leaves, but not to carbon itself. On this point, vegetable physiology has been thrown into error by incorrect information, as respects the chemical part of the phenomenon. The earlier chemists, who did not possess those extremely delicate methods of gas analysis, which are now available, gave wrong evidence in this matter. They stated that on exposing a plant to the sunshine, in contact with carbonic acid, the carbon was separated in a concrete state, the oxygen being left—but such is not the fact; by no known laws can such a change be brought about, and hence any reasoning based upon it, as to the colour of plants, is irrelevant. For when a plant exposed to the sun decomposes carbonic acid, a certain volume of oxygen disappears at the same time; in lieu of this, and in obedience to the laws which guide the transit of gases through tissues, (Jour. Frank. Inst. Vol. XVIII., p. 27) an equivalent volume of nitrogen is surrendered by the plant in return. Sometimes it is carbonic oxide which is absorbed, sometimes oxalic acid, or other compound of carbon with less proportion of oxygen. I do not here indicate from whence that nitrogen is derived, since botanists assert, that some plants contain no nitrogen at all; it may however exist in their juices, as gas exists in spring water, or may be retained in a compressed state on their surfaces, it is however a remarkable fact, that nitrogen is present, and perhaps not less remarkable, that its presence has hitherto been entirely overlooked.

118. The carbon thus taken from the acid, does not pass through the tissue of the leaf in a concrete form, or give rise to a concrete deposit; it bears with it a certain part of the oxygen with which it was formerly united, the rest being set free; the carbon and oxygen so conveyed into the plant, entering into combination with hydrogen, gives rise to the chromule verte; hence we see, that the green colour depends indirectly on the de-

composing action, that when this goes on without interruption, that is fully developed.

119. I took five pea plants out of the garden, as nearly resembling each other in size, and other particulars as might be: they had just appeared above the surface of the earth, and were beginning to put out leaves. These plants I designate by the numerals 1, 2, 3, 4, 5. Each one was planted in a small glass vessel, with a hole in the bottom for the purpose of supplying it with water, after the manner of a common flower pot. Number 1 was placed in a box, into which light passed which had traversed a solution of sulphate of copper and ammonia. No. 2, in a similar box into which light was admitted after having undergone the action of chromate of potassa. No. 3 was placed in the open air. No. 4 in a box, into which light passed which had been transmitted through sulphocyanate of iron. No. 5 was shut up in a dark closet. This arrangement was completed on the second day of May. With a pair of compasses the height of each plant was ascertained, and of that, and of the number of leaves, a memorandum was taken. In three days time an examination was made.

No. 1, had attained three times its former height, and doubled its number of leaves.

No. 2, not quite twice its former height, no new leaves, in appearance not so plump and transparent as formerly.

No. 3, twice its former size, with no fresh leaves.

No. 4, four and a half times its former size, and double its number of leaves.

No. 5, three and a half times its former size, the leaves looked yellowish.

120. It is here proper to remark, that the increase of size is not to be taken as an index of any action of the absorbing medium. Some years ago, I had occasion to notice, that rapidity of growth was greatly influenced by the quantity of aqueous gas in the atmosphere. Whether the observation possesses any novelty, I am not prepared to say, but if any one causes plants to grow in glass vessels, containing the maximum quantity of vapour which their atmosphere can hold, at the temperatures under trial, their unusual increase of dimensions, will present a strikingly remarkable phenomenon.

121. In fourteen days, from the commencement of this experiment, another examination was held.

No. 1, all its leaves of a grass green.

No. 2, of a darker green.

No. 3, green, but of a bluish tint when compared with a plant taken from the garden.

No. 4, of a bright green.

No. 5, pale whitish yellow, with no fresh leaves, but grown to thirteen times its former height, and apparently in a vigorous condition.

N. B.—With respect to No. 4, the plant under sulphocyanate of iron, I was not aware at the time of making this trial, of the singular properties of that substance in relation to light; in the course of a fortnight, which had elapsed, the solution from being of a deep blood red, had become perfectly colourless. No reliance is therefore to be placed on this result.

122. Among a number of experiments which were instituted with an intention of illustrating the same point, and which gave analogous results, it may be mentioned that the seeds of common garden cress, were caused to germinate and grow in the boxes mentioned above. And no matter what was the substance through which the light passed, the young plants after

reaching a certain size, were always green,—but those which grew in the dark had yellow leaves and white stalks.

123. The general result of these trials goes to prove, that it is not this or that species of ray, which gives rise to the colour of leaves, the absence of the chemical ray, or of the calorific ray does not appear to affect it, nor have we any direct proof that the colorific ray exercises any influence. Humboldt has stated, that in the mines of Germany, plants as the *poa annua, et compressa, plantago lanceolata*, &c., grow in recesses where the sun's light never comes, and provided hydrogen gas be present, their colour is green. In the Atlantic ocean he saw a marine plant *fucus vitifolius*, brought up from a depth of 190 French feet, where according to the calculations of Bouguer, the light was only equal to that emitted from a candle at 203 feet distance, and yet its colour was green. Decandolle mentions that artificial light, as that of lamps gives the same result; a proof that it is certainly not the chemical and perhaps not the calorific rays which cause the phenomenon.

124. Perhaps light in this case acts only as a kind of stimulus; it would be desirable to make trial of some plants whose leaves are naturally white; of this class there are several individuals; would they or would they not cause the decomposition of carbonic acid? From many indications it is not improbable that there is a variety of chemical rays, each of which brings about changes of a character appropriate to itself. As yet, we have not learned to distinguish these from each other, and are not provided with the means of effecting their separation. A remarkable observation which appears to me to be very much in point, was made many years ago, by Prof. Silliman; it has not obtained that attention which it deserves; he states, that on exposure of a mixture of chlorine and hydrogen to the *light* of a fire, an explosion was produced. I quote the fact, however, only from memory, and have endeavoured to substantiate it under a variety of circumstances, but with a want of success probably due to the absorbing action of the glass jars used, or to the nature of the light. It is desirable that this experiment should be once more repeated; it would settle an important point, that chemical rays of different characters exist. I have referred to this before in speaking of the perehilion motion of matter; for it is more than probable, that there are chemical rays not absorbable by the chromates of potassa.

NOTE.—*In the foregoing papers the reader is requested to make the following corrections of typographical errors.*

ERRATA

Sec.	line	instead of	read.
2	2	coherent	inherent
3	8	same light	sun's light
9	8	prism *a*	prism *d*
19	25	in the practical	in practical
40	8	glasses with	glasses fig. 19 with
40	10	various apertures	narrow apertures
40	16	chemist	chemists
41	1	pressing	falling
41	12	multipule	multiple
43	8	40° Fah.	110 Fah.
45	5	isometric	isomeric
48	3	$c + {}^{2}O$	$C + 2O$
48	6	$c + {}^{6}$	$C + O$
50	10	$26 + N$	$2O + N$
56	7	least	last
58	3	temperature	temperatures
58	3	moveable	invisible
58	4	moveable	invisible
59	20	laternal	lateral
68	11	Sulphocyrate	sulphocyanate
68	15	Sulphocyrate	sulphocyanate
68	23	platinium	platinum
69	24	an analysis	on analysis
71	5	hydrodide	hydriodide
72	2	coride	chloride
94	15	æquilibris	æquilibrio
99	4	natural	internal
114	1	wherever	whenever

Recent Occultations.

The immersion of Antares behind the moon's dark limb on the 10th of August last, was observed at several places in and near Philadelphia, as follows, in mean time of the places of observation. At the time of emersion the moon was obscured by clouds,

No.	Observer.	Mean time.	Latitude.	Assumed Longitude.
1	S. Sellers,	10h 17m 47s.20	39° 57′ 5″	0s.00 W. of State House.
2	S. C. Walker,	49 .09	56 54	1 .05 "
3	W. R. Johnson,	42 .79	59	5 .16 "
4	W. H. C. Riggs,	42 .99	59	5 .16 "
5	J. Gummere,	16 44 .00	40 1 12	35 .00 "
6	S. J. Gummere	43 .50	12	35 .00 "

The reductionof these observations after the method of Bessel, Astronomische Nachrichten, No. 151 and 152, has furnished the following results.

No.	m	a	b	c	d
1	— 5h 0m 31s.26	+ 1.747	— 3.087	+ 3.547	
2	31 .41	+ 1.747	— 3.082	+ 3.542	
3&4	35 .38	+ 1.747	— 3.083	+ 3.544	
4&5	1 4 .48	+ 1.747	— 3.030	+ 3.498	— 5h 1m 13s.74

Where,

m = the longitude from Greenwich + East — West, that would be deduced from the observation if the star's and moon's places as given in the Nautical Almanac were free from error.

ε = the number of seconds of arc by which the moon is more or less advanced in its orbit than its tabular place.

ζ = the number of seconds of arc by which the moon is north or south of its tabular place, measured on a perpendicular to its orbit.

η = the correction of the moon's tabular horizontal semi-diameter.

d = the longitude obtained by applying these corrections, whence

$d = m + a\,\varepsilon + b\,\zeta + c\,\eta$; assuming,

$d =$ — 5h 0m 40s for the State House, Philadelphia, the observations give,

$-9.''26 = a\,\varepsilon + b\,\zeta + c\,\eta$, and for Haverford School

$d =$ — 5h 1m 13s.74, as above.

SEARS C. WALKER.

September 6th, 1837.

Civil Engineering.

A short Historical and Topographical Sketch of the La Grange and Memphis Rail-road, Tennessee. By CHARLES POTTS, Chief Engineer L. & M. R. R.

The La Grange and Memphis Rail-road Company of the State of Tennessee, was incorporated by the Legislature of that State on the 14th of December, 1835. The charter is the first of its kind granted in this State. The capital stock of the company, designed for the main stem of the road, is fixed at 300,000 dollars in shares of 100 dollars each, which may be increased to 500,000 dollars. At the time the company was incorporated several lateral branch roads were contemplated and connected in the bill; the only one, however, which has fulfilled the requisitions of the charter, was the branch road from Moscow to the town of Somerville. To construct this branch the charter allowed an additional sum of 75,000 dollars, making the entire capital of the company 575,000 dollars. The corporate powers of the company were secured upon the subscription of two thousand shares of said stock. The rights and privileges granted to this company by the several provisions of its charter, are as full and ample as the warmest friends of the measure could have desired; it is, in fact, one of the best Rail-road Charters ever granted, and reflects the highest credit on the skill and ability of those individuals from whom it emanated.

In addition to this special act of the Legislature incorporating this company, a bill of more general character was passed by the same body on the 19th of February, 1836, entitled "An act to encourage Internal Improvements in this State." By this law it is enacted, that in all cases where any joint stock company has been, or may hereafter be incorporated by the Legislature of this State, for the purpose of constructing any work or works of Internal Improvement by means of Rail-roads, or McAdamized turnpike roads, within the limits of the same, and two-thirds of the capital stock of such company or companies shall have been subscribed by individuals; or by other incorporated companies, and in the opinion of the Governor, Comptroller, Treasurer and Secretary of State shall be well secured, it shall be the duty of the Governor to subscribe, on behalf of the State, for the remaining third of such capital stock, &c.

On the first Monday of April, 1836, the books for the subscription of stock were opened according to the requirements of the charter, in the towns of La Grange, Memphis, Somerville, &c., and in the course of ten days the whole amount required was subscribed and taken up, chiefly by the rich and wealthy planters of the counties of Fayette and Shelby. It is a matter of congratulation to the company that none of the stock was allowed to find its way into the hands of jobbers and speculators. A general feeling friendly to the road prevailed in the community from the earliest agitation of the subject, and a strong desire and determination had become established, as was clearly evinced at the time of subscription, that the undertaking should be started upon a basis which should inevitably carry it on to completion.

The preliminary arrangements in reference to the co-operation and league with the State, having been settled by the Commissioners and the Executive, the first election for Directors was held on the first of June, 1836, whereupon General Eastin Morris, Cols. Edward Cress, ——— Jones, Charles Michie, H. S. Morgan, and George Anderson, Esq., on part of the Company, and Majors Robert Lawrence, Epps Moody, and George Wyatt on the part of the State were returned duly elected. Upon the organization of the Board of Directors, General Eastin Morris was made President, John Anderson, Esq. Cashier, and Mr. Adams, Secretary. The following report which was made in the beginning of January 1837, contains an abstract of the operations of the company up to that period.

First Report of the President and Directors of the La Grange and Memphis Rail-road Company.

In compliance with the requisition of the 8th section of the act entitled "an act to incorporate the La Grange and Memphis Rail-road Company," passed December 14th, 1835, the President and Directors have the honor to submit the first Report of their official proceedings, with a detailed statement of the affairs of the company.

Books for the subscription of the stock were opened at La Grange, Memphis, Sommerville and other places on the first Monday of April, 1836, and the capital was subscribed within ten days.

An election was held on the 11th day of June, for nine Directors, and on the 15th the Board was organized and the officers elected. On the same day a resolution was adopted authorizing the President to accept the services of an Engineer and assistants to lay off the road. In obedience thereto Charles Potts, Esquire, of the city of Philadelphia, who had previously been highly recommended to the commissioners, was on the 21st of June notified by mail of his appointment to the office of Chief Engineer to the company; and he was requested to bring with him his assistants, and all the necessary implements, such as levels, transits, compasses, chains, rods, tapes and stationary, so as to be able to commence the surveys immediately on his arrival. On the 2d day of August a letter was received from Col. Potts under date of 13th July, accepting the appointment and informing the Board that he would be on the ground as soon as practicable. The difficulties, however, of procuring the requisite engineering instruments, together with the low water in the Ohio river, prevented the arrival of the corps until the 13th September. A few days thereafter the surveys were commenced, and have been prosecuted with unremitted zeal and perseverence up to the present time.

The surveys were commenced at La Grange, the corps consisting of the Chief Engineer, an assistant engineer, two target bearers, two chainmen, an axeman, wagoner, &c. On the 8th day of October the Engineer reported that he had proceeded in his preliminary surveys as far west as the old Agency or within one and a half miles of Mr. Glenn's, viz: Commencing at La Grange, near the junction of Main and Third streets, thence down Third to the Methodist meeting house, thence a northwestward course towards Mrs. Cage's, and from thence passing through the lands of Moses Frazier, Col. Edward Cress, Peachy Franklin, Mr. Kidd, Mr. Gwin, Rev. Mr. Roberts, Mr. Walker, Mr. Ford, dec'd, Messrs. Shinpoch and Moss; thence through the town of Moscow to Wolf, or *Nashoba* river, below the present road bridge. From this point passing southwestwardly through the swamp, the lands of Jno. A. Frazier, Mr. Avett, Messrs. David and Robert White, Mr. Head, Mr. Stegald, Mr. Burns', Messrs. Parrish and Burns, Mr. Thompson, Mr. Douglass, &c. and along Jernegan's Run.

On the 6th November, the Engineer made his second report, and that he had completed the preliminary surveys. Commencing at the last point of termination, and passing through the lands of Major Robert Glenn, thence along the north edge of the prairie, thence via Miles Davis', Esq., Mr. David Davis', J. Reagan's, Col. Tipton's, Mr. Ware's, Mr. Snow's, &c.; thence across Thomas' Run, to the land of Dr. Smith, thence along the Nonconnah Ridge, the Memphis road, at Collier's, or *New Haven*, thence on the south side of said road, and nearly parallel with it, along the north edge of Dewoody's land, crossing the Memphis road at Parks' old fields; thence continuing on the north of said road to the land of Mr. Baugus, thence continuing on the main ridge via Reddice's, Crain's, and Germantown. Thence on the ridge, leaving the Memphis road on the south, and after running through a mile and a quarter of woodland, nearly due west passes through the lands of Mr. Brooks, thence through the land of Mr. Winford, and crossing the Memphis road through the lands of Mr. Harrison, Hall, &c. thence on the north side of the Memphis road, through Mrs. Marlow's land and in a line nearly parallel with the Memphis road to Dr. Dunn's new field, thence by a line bearing considerably south of west to the southeast corner of Mr. Buntin's land. From thence leaving the ridge, along a division line between Maj. Dunn and the adjoining tracts of land on the north belonging to Messrs. Buntin and Wyatt, thence through the south-west corner of Wyatt's land and along the line between Bettice and Rozell, to a point due north of Bettice's dwellings, when the line diverges to the north through the middle of Rozell's land and through Carr's out field to the Pidgeon Roost road—thence on the west side of said road and nearly parallel with it to the bayou at Memphis, leaving Dr. Ragland's dwellings and out houses on the north, as also those of Dr. Dunn—striking the bayou at a point of a ravine about twenty rods south of the bridge, thence up the ravine to the landing on the Mississippi river a little south of Walker and Atkinson's warehouse.

The subjoined report marked (A.) contains the alterations, estimates and final location of the road; and to which the stockholders are referred for an able and satisfactory exhibition of the plan and cost of the entire work.

The surveys of the lateral road from Moscow to Sommerville were executed by Franklin Crawford, Esq., the Resident Engineer for that branch. The bearings, cost and estimates of which are clearly and satisfactorily given in his report appended, marked (B.)

And the Board of Directors take this occasion to say they have entire confidence in the experience, skill and ability of the Chief Engineer and his corps of Assistants, and that the surveys have been accurately and faithfully executed.

The preliminary surveys having been completed by the first of November, the next consideration of the Board of Directors was to determine upon the most suitable time for the reception of proposals for grubbing and grading the road, and procuring materials for the superstructure. And here they were met by conflicting difficulties. If they postponed the letting until the present or succeeding month so as to give ample time for foreigners to propose for contracts they would in all probability preclude the stockholders and other citizens, from entering as competitors for the undertaking, which justice and sound policy forbade. Under this view of the subject the Chief Engineer was instructed to advertise for the reception of sealed proposals until the 24th ultimo, for the letting of the whole road; thus enabling the citizens to avail themselves of the opportunity of hiring negroes during the holy-days, and giving quite a sufficient time to extend the notice through the middle and southern States.

On the evening of the 24th December the proposals were opened, and it was found that bids were made for grubbing and grading nearly the whole road, and for furnishing a great portion of the timbers; but generally at prices ranging from 50 to 100 per cent. above what are usually allowed for the execution of similar works, and about the same above the estimates of the Chief Engineer, whose judgment and experience in such matters entitled them to the highest consideration. The average cost for excavation and embankment on the Tuscumbia and Decatur road was 11 cents per cubic yard, and that in a heavy clay soil, with hard pan and rock obstructions; and the grubbing fell short of $170 per mile. With a firm belief therefore, that few if any of the propositions ought, or would be acceded to, their further consideration was postponed for the official action of the new Board of Directors.

If contracts cannot be closed with individuals shortly, at fair prices, it is evident that there is left but one course for the directors to pursue, and that is to purchase negroes, hire hands, employ superintendants, and prosecute the work forthwith, under the direction of the Chief Engineer.

A preferable scheme, however, for a speedy completion of the grubbing and grading is to let it to individuals in small contracts, if it can be economically done. But it is idle to talk about the adoption of either plan unless the most energetic steps are taken to place at the disposal of the directors the requisite funds.

By the 4th section of the charter, not more than one-fourth of the stock can be called for in any one year. And assuming it as certain that the calls will be made as soon as they can legally be, there will be due for the year ending the 15th June next, the sum of $93,750, to wit, $62,500 from individual stockholders, and $31,250 in bonds of the State, payable to the company twenty-five years after date, bearing an interest of 5¼ per cent. per annum. Of this sum there has been called only $18,750, to wit, $12,500 from individuals, and $6,250 from the State; leaving $75,000 to be paid in by the 15th June next; $50,000 in cash and $25,000 in State bonds.

Should subsequent calls be regularly made and punctually met, the whole capital stock will not be paid in, if in monthly payments, before the 15th June, 1840, more than a year beyond the time originally allotted for the consummation of the enterprise.

By a reference to the report of the Engineer it will be seen that the estimated cost of the whole road, including sidelings, water stations, buildings, damages for the right of way, salaries of officers, engineers, &c. amounts to $496,381 66, being $121,381 66 more than the original capital. But as the charter provides that if the capital stock shall be found insufficient, the President and Directors may increase the same to $575,000 for the main and lateral road. There would remain then unsold 2000 shares, which may be brought into market at any time; say $133,333 33⅓ to be taken by individuals, and $66,666 66⅔ by the State.

To avoid the hazard of reducing the present stock below its par value by a public sale of additional shares, it is suggested for the consideration of the stockholders whether a judicious arrangement might not be made with contractors, requiring them to receive one-fifth in stock, say the 20 per cent. proposed to be retained as a security for the performance of their contracts. By the adoption of this plan that amount would be paid to contractors as cash, and the company would receive in bonds from the State, half as much more.

Immediately after the organization of the Board a correspondence was opened with several extensive Iron dealers and shippers in Philadelphia and New Orleans with a view of the procurement of the requisite quantity of railway iron, &c. The proposition of Messrs. A. & G. Ralston of Philadelphia, to import the quantity required at 5 per cent. commission, one per cent. bank commission, and 6d per ton for inspection, and requiring no advancement of funds until the arrival of the iron in this country, being the best received, the President and Chief Engineer, in conformity to instructions, on the 8th day of November last, commissioned the House to procure 1493 tons of railway iron, 2½ by ⅝ inches, 15 feet in length, flat bars with countersunk holes, 10 tons 4 cwt. of splicing plates, and 816 kegs of spikes (100 lbs. each) to be delivered at New Orleans by the 1st Nov. 1837. A reply was received under date of 25th Nov. last, informing the Board that they would import the rails required provided that some arrangement could be made for the transmission of funds to London along with the order—but that it would not suit any House in America to advance the money for the purchase of that quantity and not be reimbursed for a whole year, particularly when money was worth from two to four per cent. a month. This disappointment, occasioned by the deranged state of the currency, will require the company to raise by Bills, foreign or domestic, about $130,000 during the present year, over and above the necessary calls to carry on the work.

It will be advisable also, to contract for at least two locomotive engines, and some 40 or 50 cars, to be ready by the time the superstructure is laid. Communications have been received from M. W. Baldwin and William Norris of Philadelphia, two of the most celebrated engine builders in the United States. They each proffer to furnish locomotives, at their manufactories, for $7000 a piece.

Those manufactured by William Norris, for ascending and descending inclined planes, exceed any thing of the kind hitherto attained. The "George Washington," now running on the Columbia and Philadelphia Rail Road, ascended a plane 2800 feet in length, with an ascent in that distance of 196 feet, or at a rate of 369 to the mile, or 7 feet rise in 100 feet, or one foot in 14. The weight of the engine 14,930 lbs. only. The load attached weighed 19,200 pounds, including the weight of 24 persons who were on the tender and burthen car; ascending the 2800 feet in the space of two minutes one second, or at the rate of 15½ miles per hour. Pressure on the boiler under 60 pounds to the square inch. The "Washington County Farmer," another engine built by Mr. Norris, is now in successful operation on said road. The first load drawn by her consisted of 28 loaded cars weighing 141¾ tons. The ascents very heavy; the least being 28 feet rise per mile, the majority 32, and the greatest 47 feet. Thus encumbered she passed over the steep ascent of 47 feet per mile, which is upwards of ¾ of a mile long at the rate of 22 miles per hour!

It must be gratifying to the Stockholders to perceive by the reasonable estimates of the Chief Engineer, that the annual proceeds of the road when completed will amount to about $109,500, being upwards of 20 per cent. on the entire cost. Should this calculation be correctly based, and it unquestionably is, the stock bids fair to be the best of the kind in America. For that our road will form a link in a great chain of railways extending from Memphis to Charleston does not admit of a reasonable doubt. Every recent movement in the States of South Carolina, Georgia, Alabama and Mississippi, for the accomplishment of this grand design indicates the most favorable result. Governor McDuffie in his message to the South Carolina Legislature on the 29th of November last, strongly recommends that the Charleston and Cincinnati Rail-road should terminate at the mouth of Nolechuchy in East Tennessee. In fact it is difficult to conceive what could originally have induced the citizens of that State to select Cincinnati as a point of termination, when the avowed object of the enterprise was to obtain the entire trade of the west. Why not bring the road directly to Memphis, below the mouths of all the principal branches of the Mississippi river and thus secure the whole, instead of a small portion, of the vast commerce of the Valley?

A rail-road convention, consisting of 107 delegates, representing 38 counties, was held at Macon in Georgia, on the first Monday of November last, comprising many individuals of the first talents in the State. Mr. Chappell, as chairman of a select committee of 40, reported a preamble and resolutions recommending to the Legislature of Georgia to commence a system of rail-road improvements by constructing a rail-road from a point on the Tennessee line at or near Rossville to some suitable point at or near Chattahoochie river, running through the Cherokee counties, on the most practicable route between said points, hereafter to be ascertained by Legislative provisions. And to authorize any companies that may have charters, and to create others where they do not exist, to branch from any point on said main trunk to the towns of *Forsyth*, *Athens*, *Columbia*, and such other places as the Legislature may designate. The State to build the main stem and take one-fourth of the stock in all the branches. The preamble and resolutions were adopted with but two dissenting voices. This may be considered as decisive as to the course that Georgia will pursue in reference to this subject.

It is contemplated that the main stem leading eastward, from Ross' Ferry to Chattahoochie will be about 110 miles in length, and the three branches will vary from 100, 150 to 160 miles.

From the proposed intersection at Ross' Ferry the Tennessee river is navigable at all seasons of the year for the largest steamboats to Decatur, Alabama; and thence to Tuscumbia, 43 miles, a rail-road is in successful operation. From Tuscumbia to La Grange, Tennessee, it is about 105 miles, twenty miles of which are in Alabama, 70 in Mississippi, and fifteen in Tennessee.

About five miles of the proposed Georgia road will fall in this State, but it is designed to effect an arrangement with the Hiwassee Rail-road Company, chartered by our Legislature at its last regular session. This road is to extend from Knoxville to a point in Georgia that will intersect the contemplated road from Memphis to Augusta via Athens. The stock is all taken, and John C. Trautwine, Esq. an experienced Engineer from Pennsylvania, is about to commence the preliminary surveys.

In addition to the prospects of a connection with Charleston, it should be recollected that the Legislature of this State, at the last special session appropriated $15,000 for the survey of a route from the most eligible point on the Mississippi river, as near the centre of the State as practicable, and to run east to the Tennessee river; and from thence for a McAdamized turnpike and rail-road, the most eligible route near the centre of the State, to terminate on the Virginia line, and that there is a reasonable probability that it will join our road at Sommerville.

But should these embryo schemes prove abortive, it is certain that *our road* can, with ordinary exertions, be speedily accomplished, and when completed that it will be sufficiently profitable to satisfy any reasonable stockholder.

In fine, nothing but the demon of discord, petty jealousies, and those party dissentions which will occasionally creep into the best regulated associations, can destroy public confidence, or prevent the company from carrying the enterprise to a glorious and triumphant termination.

The receipts and disbursemehts of the treasury, the amount of cash on hand, and the real and personal property belonging to the company, will be found in the Cashier's exhibit, marked (C.) Respectfully

EASTIN MORRIS,
President of the La Grange and Memphis Rail-road Company.
La Grange, Ten. Jan. 2d, 1837.

At the annual election for Directors which took place on the first Monday in January of the present year, some changes were made in the Directors.—The present Board is composed of the following members viz: Cols. Chas. Michie, President, John J. Potts, Cashier and Secretary; Majors John D. White, John C. Cooper, J. Putney, Elisha W. Harris and Harrison Locke, Directors, on the part of the Individual Stockholders, and Majors Epps Moody, George Wyatt and Robert J. Mahaffy, on the part of the State.—

In the early part of February, the whole of the work required to grade the road, with the exception of twenty miles, was let out to contractors upon very reasonable terms, ten miles of the above twenty have since been let. On the first of July last, over twenty-five miles of the road was grubbed and cleared, and several miles of the road graded. The contractors for grading were delayed considerably for the want of hands and the proper implements to carry on the work; these difficulties, however, are rapidly subsiding; the partial failure of the cotton crops in some sections of this and the adjoining State, has caused hands to be more plenty, whilst the demand for carts, barrows &c. required on the road, has induced mechanics, to locate themselves in the vicinity of this improvement, and to enter more largely into this species of manufacture.

The survey of this road, as now permanently established, begins at the town of La Grange, and from thence runs westwardly persuing the dividing ridge between the waters of North and South Forks of the Neshoba or Wolf River, crossing the latter near their confluence at Moscow. From this point the road runs nearly straight and level along the bottoms of Wolf River, about ten miles, when it strikes a spur of the Nonconna Ridge, which it rises, and thence pursues the summit of said ridge to the Mississippi River at Memphis. The whole length is nearly fifty miles. The profile of the ground is rather undulating, the fall from the town of La Grange to Wolf river is 225 feet; from this point to the Bluffs at Memphis the fall is 82 feet, and from the Bluffs to low water mark of the Mississippi river, the fall is 50 feet, making the entire descent from La Grange to the Mississippi river 357 feet. The lateral branch road which commences at Moscow, and extends to the town of Somerville, is thirteen and one half miles long. As far as it has been practicable the road has been located in a straight line, very few curves are made, the smallest being 1260 feet radius. Many of the tangent or straight lines are five mile in length—the one on Wolf river bottoms is nearly ten miles long. In profile, the road is undulating--the heaviest grades, however, which occur between La Grange and Wolf river, and in rising the Nonconna ridge, are only 40 feet per mile. The recent improvements that have been made in the arrangement of Locomotive Engines, make grades of this magnitude easy of ascent. On the Columbia road in Pennsylvania, many of the inclined planes are 40 feet per mile, and some of them 47 feet--the improved engines on this road carry the loaded trains over these grades with very little change in the velocity.

The ground over which the road runs is generally sandy loam, the larger proportion being sand; the country is entirely an alluvial formation; on the bottoms of Wolf river a slight mixture of clay obtains; no solid rock of any magnitude has as yet been discovered; on the branches of some of the streams running into north fork, and along which the lateral road passes, some traces of sand rock have been obtained, and applied to the cosntruction of drains on that part of the work; gravel is also scarce, and seldom occurs near the surface, or at such depths as it will be required to cut in most of the excavations along the road. The company is allowed by their charter to condemn 200 feet in width of ground for the use of the road. The superstructure is to be formed with timber; the first ten or fifteen miles next to Memphis will be laid with cedar; a portion of the timber for this part of the road is now under contract—the mud, or foundation, sills, are to be 3 by 9 inches—the cross-ties 8 feet long, hewn flat on one side, and to square 6 inches in the heart of the stick; to be laid down 3 feet apart from centre to centre. The string pieces, or rails, are to be 5 by 7 inches, champhered and faced with the flat bar iron $2\frac{1}{4}$ by $\frac{5}{8}$ inches; these bars to be fastened on the rails with 5 and 7 inch spikes; those 7 inches long are intended for the ends of the bars. The cost of a single track of the superstructure, laid as above, is estimated to be $4000 per mile. Bent, or trussel, bridges, will be used to carry the road over the smallest streams and ravines. A model has already been prepared for the Lattice Bridge over Wolf river, and arrangements will shortly be made to put it under contract. Culverts and drains of stone, brick and timber, will be made according to the facilities of getting either of these materials at the site required. Excellent white, and post oak, timber is found either on, or in the immediate vicinity of the whole line of the survey. Since this road has been commenced, several other similar projects have been contemplated to connect with it. A lateral branch from Salem in Tippah county, and another from Holly-Springs, Marshall county, both in Mississippi, are in agitation. The former of these branches it is confidently believed will shortly be put under construction—this branch will probably form part of the connecting link between La Grange and Tuscumbia in Alabama, and a part of the great chain of rail roads connecting Memphis on the Mississippi, with Charleston on the Atlantic. A branch road is also intended from the lower end, and designed to strike the Mississippi river near the old scite of Fort Pickering. At this point a new town has been laid out by John C. McLamore, Esq., to be called the "City of Girard." The site selected for this town, is unquestionably one of the best and most advantageous on the Mississippi river—it is situated on the summit of the Chickasaw-Bluff, and lies about midway between the waters of Wolf river and Nonconnoch. The general surface of the bluff, parallel with the river, and for a considerable extent eastward, is nearly level, and very moderately undulating. Being elevated considerably above the Mississippi river, and having no low grounds either in its front or in its rear, subject to inundations from the river, or the "Bayou Gayoso" the situation is therefore considered both pleasant and healthy. At the foot of the bluff, and along the margin of the river, a bench, or bank, extends for a considerable distance, forming the western boundary of the proposed town. This bank averages 150 feet in width, and is elevated about two feet above high water mark of the river. The bank is accessible in all stages of the water for Steam Boats, and river craft, and therefore affords the most desirable location for large mercantile houses. Memphis, which is situated a short distance above, is a place of considerable business; it is estimated that from this point and Randolph together, over sixty thousand bales of cotton are annually exported to New Orleans. This cotton is entirely the product of four or five of the most south western counties in the district of Tennessee, and the more northern counties in the state of Mississippi. The western district of Tennessee, ten years ago, was in the tenure of the Indians, and it is scarcely five years since any thing like an opening was made in it for the culture of cotton. The northern counties in Mississippi are just passing into the hands of the Planters; the Chickasaw Indians who have hitherto occupied these lands, having commenced their emigrating march in the early part of last summer. The stream of emigration which is pouring into this new country from the States of Virginia, the Carolinas, and Georgia, is beyond conception. The Chickasaw-bluff referred to above, is the only feasible point on the Mississippi river, north of Vicksburg, whereby the products of these counties can be conveyed to the river; this fact together with the fertility of the soil, and congeniality of the climate for the cultivation of cotton, with the other advantages above related, must all conduce to form at this point, one of the largest and most thriving towns on the Mississippi river.

Bibliographical Notice.

Principles of Geology; being an Inquiry how far the former changes of the earth's surface are referable to causes now in operation. By Charles Lyell, Esq. F. R. S. *President of the Geological Society of London. In* two volumes. First American from the Fifth and last London edition. Kay & Brother, Philadelphia, and J. J. Kay & Co. Pittsburg, 1837.

It is seldom that American publishers have done a better service to science than by this republication of Lyell's Geology. Coming to us with all the freshness of a new edition recently from the hands of the author, and with many valuable additions and new graphic illustrations, it cannot fail to render this new and captivating department of knowledge, additionally popular in the Seminaries and among the reading classes of this country. There is no work extant which can come in competition with it; for however meritorious and deservedly popular are the works of Bakewell and Buckland, the subject of Geology is treated by Lyell in a way and mode so distinct from these and other authors, they are scarcely susceptible of comparison. The plan pursued by the latter is so strictly that of induction (the true philosophic method of teaching whenever the subject admits of it) the reader finds no difficulty in confining his attention to the pages before him—his imagination as well as his judgment is brought into play, and he is carried from chapter to chapter by an exhibition of facts of the most interesting character, and by the allurements of a style which is at once a demonstration of a refined and classic taste, and of an understanding thoroughly impressed with the dignity and importance of its theme.

If any one should suppose that this treatise on Geology consists of a detail of formations, and their relative ages; of the order of superpositions, and the various organic remains embedded in their strata,—of the changes produced by violent catastrophes whether of flood or fire,—of the subjects which are requisite merely to convey a knowledge of geological structure,—he will form a most inadequate conception of it, either as a body of facts or a system of profound logical reasoning. It required the energies of a mind, capable of embracing, and trained to the study of, a most extensive circle of scientific and general knowledge, to grapple with the difficulties which the author had to encounter in support of his theory; and we are no less gratified with the mass of information which he brings before us, than with the good taste and judgment employed in his reasonings upon them.

We are so accustomed to consider all the great changes which the earth has undergone; the conversion of continents into oceans the elevation of high mountains above the flood of waters beneath which they had been long submerged,—the existence of extensive lakes where there are now fertile regions and populous States,--and the entire destruction of whole races of animals, some of which were of the greatest dimensions, as the work of sudden and miraculous cataclysms, affecting the whole sublunary system—we are at first astonished at the proposition, that all these great changes, and a vast multitude of subordinate ones, may have been the result of causes now in operation, and that the changes which are going on before our eyes, may eventually produce alterations of the earth's surface as remarkable as those which Geology and Natural History have ever brought to light. However startling such a proposition may be, and opposed to all our pre-existing ideas of the history of the earth and of man, there is nothing in it so revolting to the imagination, as to prevent us from listening with philosophic interest to the evidence and reasoning of our author. We discover that his hypothesis is advanced with the modesty of true science. We perceive that it bears no sort of relation to the visionary theories, for which the early history of Geology is so distinguished. The facts which he adduces deeply interest us as veritable histories of the past and the present. We discover nothing unreasonable in the opinion, that the whole valley of the Mississippi, as far up as the mouth of the Ohio, may have been formerly a great estuary which has been filled up by the materials constantly brought down by the rivers. These it is known are now accumulating in such amazing quantities as to have produced immense accessions of land even within the memory of persons now living. We are struck with the apparent justness of the conclusion (not however original with our author) that the falls of Niagara were once at Queens-town, that the mighty trough, 7 miles in length, which now constitutes the river below, may have been excavated by the constant erosion of the cataract, and that if the barrier which confines lake Erie, should thus eventually be broken away, and the waters of that and all the upper lakes be thus let loose, the State of New York and a large part of the eastern portion of our continent, might be devastated, and sunk beneath the floods. The author does not advance this as a *probable* occurrence. Lake Erie may be filled up by depositions of transported matter long ere its rocky boundaries are thus invaded, and its wide expanse be changed into fertile prairies and arable fields, with a wide and rapid river extending through them.

It is the constant tendency of rains and floods to wear down the mountains and hills, to transport their masses to the sea, and consequently to effect essential changes in the bottom of the ocean, causing continual accessions of land in places where these depositions remain long undisturbed. Oceanic currents and storms are constantly varying these relations of land and water. Subterranean fire is also a powerful element in the daily local transmutations of the earth's surface. The body of facts, in relation to all these changes, which are embraced within the brief period of authentic history, has been so skillfully arranged in the work before us, no one can read the second book which treats of these facts, without admiting the extreme plausibility of the inferences which the author draws from them. These admissions, it is true, must be granted, at the expense of our former opinions with regard to the age of the world. But the facts which have been recently so well established, and which are constantly accumulating, in relation to the changes that have taken place in the organic world, the destruction of species, the entombing of lost and also of existing species, both of marine and land animals, in solid strata of very ancient date, compel us to believe that the registry of the first chapter of Genesis may not have been clearly understood, and that there is a mode of interpretation which justifies us in our adherence both to the security and truth of the Bible and to the undeniable archieves of Geology.

In his third book the author treats of the changes in the organic world, including the geographical distribution of species, the modifications in physical geography caused by plants, inferior animals, and man; of fossil formations, of coral reefs and other matters pertaining to this new and highly curious and interesting portion of modern Geology; and in the fourth and last book, under twenty-seven distinct heads or chapters, we have a lucid account of the general arrangement of the materials of the Earth's crust. In this division of his work, the genius and industry of the author are particularly apparent. It would have been impracticable, without devoting years to travel and observation, and without an extensive acquaintance with Botanical, Zoological and Astronomical science, to furnish such a theory of structure and change, as he has here exhibited. It was in view of the peculiar merits of this part of Mr. Lyell's work, as well as in reference to its ability as a whole, that the Royal Society of London, awarded to him the Royal medal in 1834. The following were the grounds on which this distinction was confered.

"*First*, The comprehensive view which the author has taken of his subject, and the philosophical spirit and dignity with which he has treated it.

"*Secondly*, The important service he has rendered to science, by specially directing the attention of Geologists to effects produced by existing causes.

"*Thirdly*, His admirable description of many tertiary deposites; several of these descriptions being drawn from original observations.

"*And Lastly*, The new mode of investigating tertiary deposites which his labours have greatly contributed to introduce; namely, that of determining the relative proportions of extinct and still existing species, with a view to discover the relative "ages of distant and unconnected tertiary deposites."

We have omitted to say that in the First Book, after an appropriate statement of the objects and nature of Geology, the author has given a Historical sketch of its progress, in which the reader will find a succinct but satisfactory account, of the fanciful theories and ingenious cosmogonies of Buffon, Burnet, Woodward, and others, as well as of the more philosophic views of Werner, Hutton, and those of their school.

It would be almost superfluous to say that we regard the work before us as one of the most able contributions to human science, which has appeared within the present century; and that it cannot fail, in addition to the labours of others, to place Geology among the most dignified and interesting departments of general knowledge.

But,—in thus venturing to add our humble tribute to the eulogy of others, we would not be understood as expressing our opinion on the sufficiency and perfect soundness of the author's theory. Some lingering doubts, we must acknowledge, adhere to our minds of the entire adequacy of "causes now in operation, to produce all the former changes" which the earth's surface has undergone. But the candour of the author is no where more manifest, than in the putting forth of his theory, and none we presume would be more indulgent to the scruples of others.

Suggestions for the use of the Blowpipe by working Miners. By John Prideaux, Esq

Continued from p. 166.*

When a metallic bead is produced, it may be pure silver, tin, lead, bismuth, copper, gold; or iron, copper, nickel, or cobalt, alloyed with arsenic; or a mixture of various metals.

Gold and silver may be distinguished by not losing their brilliancy in the outer flame; tin by its whiteness and softness; lead and copper are immediately distinguished by their colour; and bismuth by fuming and evaporating in the reducing flame. The arsenical alloys and compounds, not clearly distinguishable by these means, nor by their streak upon the black flint, must be examined by fluxing with borax.

If it give green on pipe-clay, it is copper, though it happen to be bleached by the arsenic.

If blue, it is cobalt.

If orange-yellow while hot, and the colour fly on cooling, it is iron or nickel; and these are distinguished by the reducing flame on charcoal, where the borax bead is bottle-green with iron, but almost colourless with nickel.

If the metal yield no bead, but pass off in vapour, it is quicksilver, arsenic, antimony, bismuth; or, possibly, tellurium, cadmium, or zinc.

If the vapour smell strongly of garlic, it is arsenic.

If it leave a circular halo on the charcoal, it is antimony, tellurium, bismuth, or cadmium.

If the halo be white, it is antimony.

If orange-yellow, it is to be subjected to the reducing flame. If it disappear easily, tinging the flame green, it is tellurium.

If it evaporate with difficulty, and without tinging the flame, it is bismuth.

If it be red, or orange-red, it is cadmium.

If the mineral evaporate readily with no odour, or only that of sulphur, and without leaving a white or yellow halo, it probably contains quicksilver; which is proved, if, on mixing a portion of it with soda and iron filings, heating it on charcoal, and holding a bit of gold coin on the vapour, the quicksilver will show itself on the coin.

If, using soda with the reducing flame, the assay burn, after a while, with a pale-green flame and white smoke, covering the charcoal with a white flaky powder, it contains zinc.

If it will neither yield a bead of metal nor volatilize, but attract the magnetic needle after the operation of the reducing flame, it most probably contains iron; but it may be nickel or cobalt: easily distinguished by fluxing with borax.

If the mineral, or the residue, after part has evaporated, will neither yield a bead, volatilize, nor attract the magnetic needle, we have then to flux it with borax; and the following table will show what it contains:—

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* Erratum. At page 161 third line from bottom for Paideaux, read Prideaux.

If it stain the borax In the reducing flame, on charcoal.	 In the calcining flame, on pipe-clay.	The metal is	Estimation.
Blue	Blue	Cobalt	Valuable, for Colouring glass, &c.
Bright green	Colourless	Chrome	Ditto, and for Paints.
Colourless, or reddish	Bluish green	Copper	Valuable.
Bottle-green	Orange, while hot; Bleaches in cooling.	Iron	Of no value unless near coal.
Pale	As iron	Nickel	Valuable.
Dirty green	Yellow	Uranium	Not used.
Purple	Colourless	Titanium	Ditto.
Colourless	Purple	Manganese	Valuable, for bleaching, &c.

Copper is reduced to the metallic state, in the reducing flame; and hence, when in quantity, shows itself in its usual red colour, in the bead.

In mixtures of metals, the indications are sometimes ready enough. If on pipe-clay, we find the borax tinged between orange and purple while hot, and becoming purple on cooling, whilst it gives a bottle-green on charcoal, we immediately perceive the indications of iron and manganese; a very common mixture.

If again, we find it bright green on pipe clay, and emerald-green on charcoal, a mixture of chrome and copper is indicated.

But it more frequently happens that mixtures of metals give ambiguous results; and that they can only be ascertained by caution and perseverance.

Although most of this is sufficiently easy, it requires a little practice: and, to give the operator confidence in his results, this is best performed upon substances which he knows to contain the metals he assays for. Thus, if he operate on common mundic, he is certain, after a sufficient time of roasting and heating on charcoal, to obtain a residuum capable of affecting the magnetic needle. White mundic will leave the same residue, after giving off an arsenical smoke. Green copper (malachite) will be sure to yield a bead of copper on charcoal, and will as certainly produce a fine green with borax on pipe-clay. Any of the ores of lead may be promptly reduced to a metallic bead on charcoal.

The vapour of antimony may be easily distinguished from that of arsenic by the garlic odour of the latter.

A minute particle of manganese will tinge borax a fine purple, on pipe-clay, in the outer flame; but on charcoal, in the blue flame, a much larger portion, dissolved in borax, will become limpid and colourless. A particle of titanium ore will give an opposite result, bleaching on the pipe-clay, and becoming purple on the charcoal.

The experimenter should also not fail to reduce tin ore by the aid of soda, and some ore of zinc by the same means, that he may become acquainted with the appearances in these assays.

Thus varying his practice as materials happen to fall in his way, he will quickly acquire familiarity with the appearances and results, and feel confidence in any assay he may undertake.

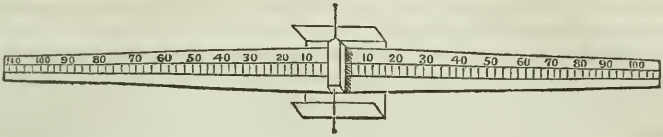
Should he wish to carry his investigation farther, and ascertain the proportion of metal contained in the ore, this may be done on a small fragment of any thing which yields a regulus; and by an instrument so simple that a clever workman may make it for himself.

The common marsh-reed, growing in such places, generally, throughout the Kingdom, will yield straight joints, from eight to twelve, or more, inches long. An eight-inch joint will serve, but the longer the better. This joint is to be split down its whole length, so as form a trough, say a quarter of an inch wide in the middle, narrowed away to $\frac{1}{8}$ of an inch at the ends. A narrow slip of writing-paper, the thinner the better (bank-post is very convenient for the purpose,) and as long as the reed trough, is to be stuck, with common paste, on the face of a carpenter's rule, or, in preference, that of an exciseman, as the inches are divided into tenths instead of eighths; in either case observing that the divisions of the inch on the rule be left uncovered by the paper. When it is dry, lines must be drawn the whole length of it, $\frac{1}{8}$ of an inch apart, to mark out a stripe $\frac{1}{8}$ of an inch wide. Upon this stripe the divisions of the inch are to be ruled off, by means of a small square.

The centre division being marked O, it is to be numbered at every fourth line, to the ends. Thus the fourth from the centre on each side will be 10; the 8th, 20; the 12th, 30; the 16th, 40, &c.; and a slip of 10 inches long, graduated into 10ths of an inch, will have on each arm 50 lines, or 125 degrees, divided by these lines into quarters. While the lines and numbers are drying, *the exact centre* of the reed-trough may be ascertained, and marked *right* across by spots on the two edges. A line of gum-water, full $\frac{1}{8}$ of an inch wide, is then laid, with a camel's-hair pencil, along the hollow; and the paper being stripped from the rule (which it leaves easily,) the graduated stripe is cut out with scissors, and laid in the trough, with the line O exactly in the centre. Being pressed close to the gummed reed, by passing the round end of a quill along it, it graduates the trough from the centre to each end. This graduation is very true, if well managed, as the paper does not stretch with the gum-water, after being laid on the rule with paste.

A very fine needle is next to be procured (those called *bead*-needles are the finest,) and passed through a slip of cork, the width of the centre of the trough, about $\frac{1}{4}$ of an inch square, and $\frac{1}{8}$ thick. It should be passed through with care, so as to be quite straight. The cork should then be cut, until one edge of it fits into the trough; so that the needle shall bear on the edges, exactly in the spots that marked the centre, as it is of importance that the needle and trough be exactly at right angles to each other. The cork is now to be fixed in its place with gum-water, and, when fast dry, to be soldered down on each side with a small portion of any soft resinous cement, on the point of a wire or knitting-needle. A little cement being also applied in the same manner to the edges of the cork where the needle goes through, to give it firmness, the beam is finished. It may be balanced by paring the edges on the heaviest side; but accurate adjustment is needless, as it is subject to vary with the dampness or dryness of the air.

The support on which it plays, is a bit of tin plate (or, in preference, brass plate) $1\frac{3}{8}$ inch long and 1 inch wide. The two ends are turned up square $\frac{3}{8}$ inch, giving a base of $\frac{5}{8}$ inch wide, and two upright sides $\frac{3}{8}$ high. The upper edges are then rubbed down smooth and square upon a Turkey stone, letting both edges bear on the stone together, that they may exactly correspond. For use, the beam is placed evenly in the support, with the needle resting across the edges. Being brought to an exact balance by a bit of writing paper or any other substance placed on the lighter side, and moved toward the end until the equilibrium is produced; it will turn with extreme delicacy, a bit of horse-hair $\frac{1}{8}$ inch long being sufficient to bring it down freely.



This is used as follows.

A suitable fragment for an assay of the mineral to be tried is placed on one side of the beam, and counterpoised by a small weight, or any other substance, on the other side: on No. 100, if the produce be required per cent. (but if per cwt., on the line beyond No. 110;) the assay piece being moved to or from the centre, until the balance is adjusted. Its exact place on the beam being then noted, it is to be taken off and reduced in the usual manner.

When the reduction is complete and the particles of metal have been brought together into one bead, it is allowed to cool, and, being broken out of the soda (if any was used,) is replaced on the beam precisely where it stood before.

The counterpoise will now require to be moved toward the centre, in proportion to the loss of the assay. The number on which it stands, when the balance is restored being multiplied by 10, will give the produce per cent. or per cwt., according to the first position of the counterpoise.

A beam of this kind is described in the "Annals of Philosophy," of a graduated thin slip of deal, the needle fixed on with sealing-wax; but this is rather troublesome to graduate, the polish of the needle is apt to be impaired by the heat; its greater weight also interferes with its delicacy, and a blowpipe bead is very subject to fall off its flat surface. On these accounts, I find the reed beam an improvement. Brass plate is, however, where procurable, preferable to tin plate for the supports: and still much better, two straight bits of fine glass rod or tube, fixed with cement on the edges of a groove, in a piece of wood of the requisite dimensions.

The utility of the information contained in this article, both to the miner and the student in mineralogy, induces us to add a few remarks on the same subject, in order that it may be complete in the present number.

Trusting that our endeavours to facilitate the rudiments of this interesting study, will meet the indulgence and assistance of our more scientific readers, we shall proceed to observe, that as the lamp described by Berzelius, or one of a similar kind, may be almost as easily procured as the miner's candle, it will be found of some importance to employ it in preference, in all cases where it is attainable. It is necessary that the operator should perceive distinctly the conical flame and transparent vapour emanating from the blast; and this desideratum will be effected with much more convenience by using a lamp.

The wick should be cut obliquely, the most elevated part being on the right hand; a longitudinal opening being made for the insertion of the blowpipe; care must be taken to use oil which has not been purified by sulphuric acid, for the reason that the acid will always remain in sufficient quantity to effect the texture of the wick, and change the colour of the vapour produced. The cotton should also be in its raw state, or a similar effect may be produced by the presence of the chloruret of lime, which is sometimes used in the process of bleaching.

Until the flame can be well and readily distinguished by the operator, a darkened room will be the best adapted for his experiments; he will soon observe that, when the assay is brought in contact with the interior flame, the surrounding vapour will change its colour from blue to an orange-yellow, the intensity of which will depend on the nature of the substance submitted to the action of the flame. This vapour gradually diminishes and disappears, and the assay becomes surrounded by an atmosphere of a scarcely visible blue tinge, or takes a colour peculiar to the assay which is volatilized at this period of the process.

The carbonate and sulphate of strontian give, at first, a pale vapour, which is succeeded by a most brilliant and permanent red. The same colour is also produced from fluor spar, more intense, but of less brilliancy, and a weaker tinge is given out from the sulphate of lime; arsenic colours the outward flame of a clear blue; antimony somewhat deeper; and lead, a fine azure; green is evolved from boracic acid; minerals containing barytes communicate a paler tinge, but the colour does not become apparent until the assay begins to melt, after which, it increases in brightness and beauty for a considerable time; borax has, at first, a dense reddish atmosphere, and does not exhibit the green colour unless sprinkled with sulphuric acid; minerals containing copper, even in very small quantity, produce a fine green at the extremity of the blue jet; lead ores, holding copper in combination, have a similar appearance, the jet being in this case of a finer blue. The presence of potassium is discovered by fusing a portion of oxide of nickel with borax, and submitting the compound, in conjunction with the assay, to the action of the flame; if it contain potassium, a mass of blue glass will be the product.

These and the preceding indications will be a sufficient guide to the student in very many cases, and his own observations on the process during the experiments will enable him to pursue his study with pleasure, and, possibly, with profit.

Mining Review.

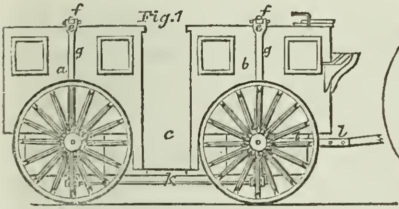
English Patents.

Specification of the patent granted to Thomas Routledge, *and* Elijah Galloway, *of the City of London, for Improvements in Cabriolets and Omnibuses.*—Sealed December 19, 1836.

Figs. 1, 2, 3, 4 exhibit our improvements in omnibuses. The body instead of having straight and parallel seats with an entrance behind is divided into two equal parts, *a*, *b*. The passengers, therefore enter, at either side into the space, *c*, and through the doorways, *o*, *o*, sitting on the circular seats, *d*, *d*, *d*, *d*, as will be clearly seen by the sectional plan, fig. 5, and which, therefore, it is unnecessary more minutely to describe. The wheels are all of equal size, and in turning the carriage they all lock; thus differing from other four-wheel carriages, the fore-wheels only of which can lock.

The mode in which this improvement is effected may be thus explained, *e*, *e*, are two springs on the top of each body, which springs have a hole through the centre, or are fitted with a socket secured to them by a clip or other well known means, turn freely on the pivots, *f*, *f*, which pivots are securely fixed on the top of the bodies, *a*, *b*; the ends of these springs are also securely attached to the vertical bars, *g*, *g*, to which vertical bars are welded or otherwise securely affixed, the arms, *h*, *h*, upon which the wheels revolve; underneath the carriage there are also springs, *i*, *i*, of corresponding dimensions with those above the carriage, viz., *e*, *e*. The two springs, *e*, *i*, and the vertical bars, *g*, *g*, form together the parallelogram, *g*, *e*, *f*, *g*, *i*, *h*, fig. 2, which turning on the pivots, *f* and *j*, can be made partially to revolve round the imaginary axis, dotted red in fig. 2; and of course with the parallelogram; the wheels upon the arms, *h*, *h*, will also move round the said imaginary axis. Now turning to the diagram, fig. 4, which represents a plan of the omnibus as if it were turned upside down, there will be seen a connecting-rod, *k*, attached to pins on the springs, *i*, *i*, there being holes in the connecting-rod ends so that it may move more freely on them. If, then, the parallelogram and wheels No. 1, (fig. 4) be moved into the position of the blue dotted lines, then the parallelogram No. 2, would be also moved on to the position of the blue lines, there shewn, and, therefore, the wheels would assume the position there shown by the blue lines; or if the parallelogram, No. 1, be moved in the opposite direction, into the position of the red lines, all the parts would assume the positions shown in red lines. The pole is attached to the frame, *l*, *l*.

Fig.1



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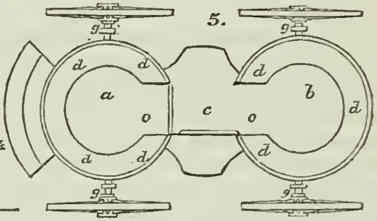

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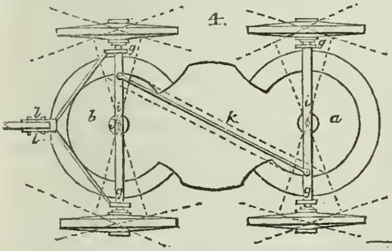

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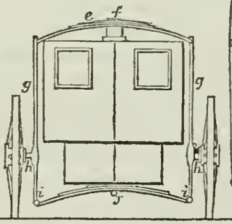

3.

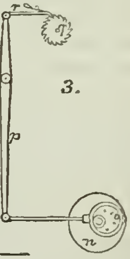

Fig. 3, is a diagram which explains the principle of our indicator for ascertaining the distance traveled by a cabriolet or omnibus. *n*, is the nave of the wheel to which the eccentric, *o*, is affixed on the end nearest the carriage; it will be readily understood that each revolution of the wheel communicates a reciprocating motion to the lever, *p*, which again gives motion to the pall, *r*, which pall moves the ratchet-wheel, *q*, one tooth for each revolution of the wheel. This ratchet is fixed to some part of the body of the carriage, and communicates its motion to a train of wheel-work well known in similar instruments, and which causes a hand or pointer to record the number of revolutions made by the wheel; and the diameter of the wheel being known, the index can be graduated so as to show the distance (in miles, yards, or other known distances) which the carriage moves over.

And, lastly, in order that we may be clearly understood as to what constitutes our improvements in cabriolets and omnibuses, we hereby declare that we lay no claim to the invention of such parts of the cabriolets as have been previously used and are well known: but we claim in them an improved combination of parts, some of which are new and some old, which combination of parts as a whole gives superior safety, as well as other advantages.

And in regard to the omnibuses herein described, we claim as our invention the dividing the body into two parts, whereby passengers may enter at either side of the platform between the said bodies. We claim also the mode of suspending the body and locking the wheels in the manner described, and we claim, finally, the application both to the cabriolets and the omnibuses, of the indicator for measuring distances, though we wish it to be understood that we do not claim the invention of such an instrument, but merely its application in the manner described and for the purpose intended.

Rep. Pat. Inv.

Specification of the Patent granted to David Mushet, *of Coleford, in the County of Gloucester, Iron Master, for a certain Improvement in the art of making or manufacturing Bar Iron or Malleable Iron.*—Sealed October 22, 1835.

My improvement in the art of making or manufacturing bar-iron or malleable-iron relates to that part of the said art which is commonly termed the puddling process, being the process now most generally practised for converting crude or cast-iron into malleable-iron, by the action of heat and flame, in a particular kind of air-furnace, called a puddling-furnace. A patent was granted to Mr. Henry Cort, in the year 1784, for what is now termed the puddling process as aforesaid, and for clear comprehension of the meaning which I attach to several technical terms which must be used in this specification, I wish it to be understood that the term cast-iron is used to designate all the various qualities of crude-iron which are obtained from the smelting of ores of iron otherwise called mine, by the operation of what is called the blast-furnace, which operation produces fusible metal capable of being melted again, and which is commonly called cast-iron, because it can be cast by pouring it when in a fluid state into moulds; it is also called pig-iron or sow-metal, from the shape of the rude ingots into which it is cast when it is run out of the blast-furnace. Such crude or cast-iron is generally admitted to be iron combined with carbon, and it is supposed to derive fusibility from the carbon which it contains, and the various degrees of fusibility of cast-iron of different qualities are supposed to be very greatly influenced by different proportions of carbon being combined with the iron. The different qualities of cast-iron are designated in trade by numbers. Nos. 1, 2, 3, and sometimes 4; the highest number being considered the best quality and the most valuable, being the most fusible and supposed to be the richest in carbon. Nos. 1 and 2, otherwise called dark grey iron from the colour which it presents when newly broken, and No. 3 (which is called grey iron) are all used by iron founders for making castings, and hence are all called foundry-metal. The crude, or cast-iron, or pig-iron, which is made by the blast-furnace expressly for the purpose of being afterwards converted into malleable-iron, is commonly called forge pig-iron, or bright grey iron; it is considered inferior in quality to either the Nos. 1, 2, or 3, used by iron founders, being less fusible than they require it to be, and it is supposed to contain less carbon; also a still less fusible quality of cast-iron than the bright grey, and supposed to contain less carbon, is occasionally made by blast-furnaces for conversion into malleable-iron; it is called mottled cast-iron from the colour of the fracture; and lastly, a very inferior quality called, from the colour of its fracture, white cast-iron is sometimes produced by the blast-furnace; it is more difficult of fusion, and is supposed to contain less carbon than the other qualities, and requires to be mixed with cast-iron of richer qualities. Before such cast or crude-iron called forge-pig or bright grey iron, or else mottled iron as aforesaid, is submitted to the puddling process before mentioned, for the purpose of being converted into malleable iron, the crude-iron is usually prepared for that process by a previous operation of melting in a furnace called a running-out-fire or furnace, otherwise called a finery, coke being used for fuel, and the fire being blown by a blast which is directed downwards upon the melted metal. After being so melted and run-out or refined the prepared cast-iron is called refined metal; now, in Staffordshire, it is called plated. By the operation of the running-out-fire or finery, the iron is supposed to be deprived of a portion of the carbon which it contained in the state in which it was first obtained from the blast-furnace, which deprivation is supposed to be owing to the dissipation or burning away of that said portion of the carbon in the running-out-fire or refinery, and the refined metal or plate becomes less fusible than it was before, which is a desirable preparation for undergoing the puddling process. Note, the aforesaid refining of crude, or cast, or pig-iron, by the running-out-fire, or finery, as a preparation for its conversion into malleable iron by the puddling process, was introduced after Mr. Cort's patent, and is not stated in his specification, but it has been the general practice for many years. The aforesaid different qualities of crude, or cast, or pig-iron, which are obtained from the blast-furnace, such as grey, mottled, or white, are mixed together when put into the running-out-fire or finery, in order to obtain a medium quality of refined metal or plate, and approximate as near as can be done towards uniformity in quality of the refined metal or plate, which is to be treated in the puddling-furnace, so as that operation may be conducted with regularity. The operation of the running-out-fire or finery is so conducted as to act more or less intensely upon the crude-iron which is subjected to it, according as previous experience has pointed out to be the most suitable, by way of preparing each quality of crude-iron, or each mixture of different qualities thereof, for conversion into malleable-iron in the puddling-furnace. The iron under these circumstances is supposed to be deprived of carbon until only such a portion remains combined with it as will render it most susceptible of conversion. When the running-out-fire or finery is managed so as to operate in its least degree upon the crude-iron (and as is supposed has had the least effect in depriving it of carbon,) the refined metal or plate which results, is said to be under blown, or it is said to be full blown, when the running-out-fire or finery operation has been carried further, and (as it is supposed) the metal has been deprived of still more of its carbon; crude or cast-iron so prepared, and called as aforesaid refined metal or plate, is said to be either under blown, or fully blown, or to be refined in any such intermediate degree between those extremes, as is found by daily experience to be most suitable to the quality or mixture of qualities of the crude-iron, for rendering it susceptible of being converted into malleable-iron by the puddling-process. The puddling-furnace is of the kind called an air-furnace, and a suitable quantity or charge of the refined metal or plate, is heated therein by a strong fire of pit-coal, the flame thereof being urged by the draft caused by a high chimney to impinge upon the charge of metal, which becoming partially melted, or brought into a semi-fluid state, is then continually stirred up and turned over, in order that the current of flame may act equally upon all parts of the semi-fluid mass, which, after some continuance of the action of the flame, commences a sort of ebullition or effervescence from the effect of intestine motion called fermentation, during which the semi-fluid, called metal, appears to glow with increased heat, and a blue flame is emitted; the stirring and turning over is continued vigorously during the fermentation, in order that the whole mass may be subjected to a like action of the heat, until, by continuance thereof, the semi-fluid metal thickens by degrees, loses its fusibility, and becomes converted into malleable-iron, the actual conversion being termed by workmen, brought into nature; and it is supposed to be effected by the dissipation and burning away of the carbon which the iron still retained after the refining operation of the running-out-fire or finery. As the conversion proceeds and becomes complete, the iron, losing its fusibility, is said to become dry when it retains no fluidity, but gets into a crumbling or flaky state, and then the charge of iron is collected into masses or lumps of a convenient size, and the fire being urged for a short time to bring the metal to a white or welding heat, the masses are in that state withdrawn one after another from the furnace, and welded solid by blows of a heavy forge hammer, called a stamping or shingling hammer, and the rude mass of solid malleable-iron so obtained, before it loses its heat, is passed repeatedly between rollers, in order to reduce and flatten it to the shape of a rough bar, such bars are called puddled-bars of malleable-iron. The hammering is sometimes dispensed with, and the masses of hot iron when withdrawn from the puddling furnace, are passed between rough rollers to effect the welding, and then by continuance of the rolling the iron is formed into a puddle-bar. The rough puddle-bars so obtained are afterwards cut up into short lengths, and the pieces piled one upon another to form suitable masses called piles, which are heated to a welding heat, or as it is termed, re-heated in another air-furnace called a balling-furnace or re-heating furnace, and when withdrawn therefrom are welded together into a solid mass, by passing between rollers, and by continuance of the rolling, the iron is formed into a finished bar of malleable-iron. And note, it has become a common practice in South Wales, to revive or smelt what is called slag-cinder or scoria of iron, resulting from the waste which the iron undergoes by oxidation in the several processes of the art of making or manufacturing bar-iron or malleable-iron, as hereinbefore enumerated, that is to say, slag-cinder or scoria, resulting from oxidation of the heated iron, either in the operation of refining the crude-iron, in the running-out-fire or finery, or in the subsequent conversion into malleable-iron, in the puddling process, or in reheating in the balling or reheating furnace, or in the operation of the forge-hammer or rollers for welding the iron into solid masses, and forming those masses into puddled-bars or finished-bars. Such slag-cinder or scoria being mixed with the native ore of iron or mine, which is put into the blast-furnace, the iron in the slag-cinder or scoria becomes revived or smelted along with the iron contained in the native ore or mine, so that the slag-cinder or scoria, which would otherwise be wasted, becomes a substitute for a part of the native ore of iron or mine, which would otherwise be required, but such use of slag-cinder or scoria being found to impair the quality of the iron which is so obtained, it is commonly found requisite in treating the same, to carry the refining operation of the running-out-fire, or finery to its full extent, in order that the refined metal or plate may be full blown as a preparation for its subsequent conversion into malleable-iron in the puddling-furnace. The use of slag-cinder or scoria as aforesaid, in the blast-furnace in admixture with native ore of iron or mine, in lieu of a portion of the ore or mine which would otherwise be used, produces a fusible quality of cast, or crude, or pig-iron, although otherwise inferior in quality; and it is not found desirable to have so great a degree of fusibility in the metal which is subject to the puddling process, because as before mentioned the metal is partially melted or brought to a semi-fluid state, when the aforesaid fermentation is excited, and the conversion into malleable-iron is effected. If the metal is too fusible when first put into the puddling furnace it will be quite melted therein, and must remain a longer time exposed to the action of the current of the flame, in order to thicken and acquire that consistency which is favourable for the commencement of the fermentation. The treatment of the metal in the running-out-fire or finery, is endeavoured to be carried to that extent called full-blown, or to any intermediate state between that and what is called under-blown, which is found by experience to be adapted to the quality of the crude or cast, or pig-iron, or mixture of qualities thereof, which is put into the running-out-fire or finery, so as to obtain refined metal or plate, which in respect to fusibility, conjointly with other qualities, is found most susceptible of conversion into malleable-iron in the puddling-furnace.

All the above particulars are fully known to persons conversant with the iron manufacture, but what is herein-before stated, contains no part of my invention, and is only introduced by way of explanation of the meaning which I attach to the terms which I use in describing my invention.

My said improvement in the art of making or manufacturing bar-iron or malleable-iron, consists in adding to the iron which is undergoing the puddling process in the puddling-furnace, rich native ore of iron in a state of powder, throwing suitable small quantities thereof at a time into the puddling-furnace upon the semi-fluid iron, so that the said powder will become intermixed with the said iron, in consequence of the manipulation or action of turning over and stirring up, which the semi-fluid iron must receive during the usual course of the puddling process; or together with the said powder of rich native ore of iron, I use charcoal, or else coal, reduced to a state of powder and intimately mixed with the powder of ore, and suitable small quantities of such powder of rich native ore of iron, either with or without powder of charcoal or coal, being from time to time added to, and intermixed with, the semi-fluid iron as aforesaid, previous to the commencement of, and during the progress of, the effervescence or fermentation hereinbefore mentioned, which precedes and accompanies the conversion of iron into a malleable state, the said fermentation is increased and the thickening of the iron is accelerated, and its coming into nature, or conversion into the malleable state is facilitated so as in the result to effect an improvement in the making or manufacturing of bar-iron. The beneficial result obtained by the improvement being in some cases, that a given weight of bar-iron can be made, by aid of the improvement, from a less quantity of crude, or cast, or pig-iron, than would be required in the ordinary course of proceeding of refining the said iron, in the running-out-fire or finery, and then puddling the refined metal or plate without the improvement; the quality of the bar-iron being as good in one way as the other, or in other cases the beneficial result will be in making bar-iron, by aid of the improvement, of a superior quality, to that which would be produced from the same crude, or cast, or pig-iron, by the ordinary course of proceeding without the improvement, the weight of crude, or cast, or pig-iron, required to make a given weight of bar-iron, being the same in one way as the other; or in other cases the beneficial result will be in making bar-iron, by aid of the improvement, of a better quality from a smaller proportion of crude, or cast, or pig-iron, than is done by the ordinary course of procedure without the improvement.

My said improvement, consisting as aforesaid, in adding rich native ore of iron in powder, either with or without powder of charcoal or coal, and mixing small quantities thereof, from time to time with semi-fluid iron which is undergoing the puddling process, so as by such addition and mixture to increase the fermentation and accelerate the conversion of that iron into the malleable state. My said improvement is supposed by me to operate, by a tendency which the rich iron ore may have to facilitate and expedite the separation of carbon, from the semi-fluid iron, when the said ore is very suddenly heated by its application in small quantities, and in a divided state, to the iron, which is already in a semi-fluid state, and consequently very hot, and that heat kept up by the continual action of the flame of the furnace. The quantity of rich ore which is thrown in at once being small, its tendency to lower the heat of the semi-fluid iron may be overcome in a great degree by keeping up the fire strongly; and by the turning over and stirring manipulation the powder is intimately mixed with the whole mass of semi-fluid iron as quickly as possible, and then the ore becoming suddenly and strongly heated, I suppose it may facilitate and expedite the extraction of carbon from the semi-fluid iron, for the addition of powder of iron ore, according to my improvement, visibly promotes and assists the intestine motion and effervescence or fermentation which the semi-fluid iron usually undergoes, and which is supposed to be occasioned by the departure of the carbon from the iron, during its conversion into the malleable state. But note, as the aforesaid supposition of the ore operating beneficially by facilitating the extrication of carbon does not admit of direct proof by any experiment which I have made, the same is only offered as a supposition in explanation of the mode whereby my improvement may produce those beneficial results which I have obtained from it, in many repeated trials on a large scale of actual practice. The iron which is contained in the rich ore, which according to my improvement is added to, and mixed with the semi-fluid iron, tends as far as it goes to increase the quantity of malleable-iron beyond that which would otherwise be produced without my improvement, and also tends to improve the quality of the malleable-iron, and give the bars thereof a more fibrous texture than they would otherwise acquire; and for the full explanation of the manner of performing my said invention, I will now state the mode of procedure, which I have found by experience to be most beneficial for making or manufacturing bar-iron or malleable-iron by aid of my improvement. The ore of iron which I employ for my improvement, may be of any kind which is rich in iron. I have used the rich-iron ores obtained in the forest of Dean, in Gloucestershire, called brush-ores, and which are considered to be of the nature of hydrous-oxides of iron, containing about sixty per cent. of their weight of metallic iron; and I have also used the rich Cumberland iron-ores, called blood-stone, or haematites, from their red colour, and which are considered to be of the nature of oxides of iron, containing more than sixty per cent.

of iron; and I have also used the rich Lancashire iron-ores, which are considered to be of the same nature as the Cumberland ores, but generally containing less iron, being from fifty to sixty-three per cent. There are many kinds of rich iron-ores, obtained in foreign countries which would answer the same purpose, but as it is not usual to import such ores of iron in quantity into this country, I have not had an opportunity of making trial of any foreign ore of iron, excepting one kind brought from the East Indies, and which is used there for making the iron from which the East Indian steel called wootz is made; but inasmuch as such rich foreign ores of iron would be substitutes for those obtained, as aforesaid, from Gloucestershire, Cumberland, or Lancaster, such rich foreign ores of iron, as well as other rich ores of iron which may possibly be obtained from other parts of Great Britain than those which I have enumerated (although as yet unknown to me,) will I expect be available for carrying my invention into effect, as the same does not depend upon any peculiarity of the ore of iron which is used, provided that the same is rich in iron, and free from (or separated from) any minerals which are inimical to the reduction, by action of heat, of the iron contained in such ores into a metallic state, and also provided that the said rich iron-ore is capable of being broken or ground to a state of powder. The rich ore of iron which I use is to be reduced to powder small enough to pass through a wire sieve or riddle, containing about 300 holes or meshes in each square inch, but the finer the powder the better it will operate, and hence if the nature of the ore will permit of reducing it easily to a fine dust, it will be advisable to do so. The ore may be broken and powdered fine by stampers of the same kind as are commonly used in Cornwall for reducing tin ores to powder; or it may be ground by rolling stones, called runners, on edge, such as are used in the making of gunpowder; or the iron ore may be crushed between iron rollers, or it may be ground fine by the same kind of machinery which is commonly used by iron-founders for grinding charcoal to make what they called moulder's blacking for applying to the moulds in which iron is to be cast, and which machinery is well known to all persons conversant with the iron manufacture; or the iron ore may be first crushed or broken small by one kind of machinery, as aforesaid, and then ground to fine powder by other kind of the said machinery, as is most suitable for getting the iron-ore into a state of minute division. The rich iron-ore which is used may be prepared by previous calcination or roasting; but I have not found any sensible difference in the efficacy of the powder of Gloucestershire, Cumberland, or Lancashire ores, when the same have been prepared by wasting, or have been reduced to powder without wasting; nevertheless, if a moderate wasting will facilitate the breaking and grinding of the ore to powder, it may be advisable to do so, but whether that will be the case or not will depend upon the texture of the particular ore which is used, and the nature of the machinery which is employed for breaking or grinding the ore, and in case powder of charcoal or coal is to be mixed with the powder of rich iron-ore, the same is to be ground fine by any of the machinery before mentioned, that which is used for grinding moulder's blacking being very suitable, and either charcoal of wood or what is called animal charcoal may be used, the latter being commonly made from bones, or from the hoofs of animals, or scraps of leather. The proportions in which I have found it advisable to use rich ore of iron in powder in the puddling process, according to my improvement, are as follows: to make common bar iron by aid of my improvement I charge the puddling furnace with about four hundred and fifty pounds weight of crude, or cast, or pig-iron, varying in quality from between what is designated bright grey and mottled approaching to white, mixing together such portions of the different qualities which are within that range of variations as previous experience has pointed out, for producing an average quality in a long series of successive charges, so as to bring the operations of converting those successive charges into malleable-iron to regularity of repetition.

And note, the selection of those said qualities, and the proportions in which they are to be mixed together, and the said averaged quality which it is desirable to produce, must be regulated by the same judgment and discretion which is commonly exercised by iron masters in selecting and mixing the crude, or cast, or pig-iron, which is to be put into the running-out-fire or finery, when they intend to prepare the same for making common bar-iron without the aid of my improvement; such judgment and discretion can only be acquired by experience, and cannot be communicated by writing, because of the numerous gradations of quality in crude, or cast, or pig-iron, which do not admit of accurate description, but it is sufficient to say that the aforesaid charge of about four hundred and fifty pounds which I put into the puddling-furnace, may be of the same quality or mixture of qualities of crude, cast, or pig-iron as is usually put into the running-out-fire or finery, in order to be prepared for making common bar-iron, but when the same bar-iron is to be made by aid of my improvement, the previous preparation of refining by the running-out-fire or finery may be dispensed with, and the crude, or cast, or pig-iron charged at once into the puddling-furnace, as was formerly practised by Mr. Cort, during the term of his patent of 1784. The management of the puddling-furnace is to be the same as usually practised (that is to say:)—before it is charged the furnace must be brought to a proper heat, and the iron bottom of the furnace prepared in the usual manner for the reception of the charge, by laying on a proper quantity of slag, cinder, or scoria, which has been separated from previous puddle-balls or re-heated piles of puddle-bars by operation of the forge hammer or rough rollers; but, note, after the charge of iron is put into the puddling-furnace with its iron bottom so prepared, no more slag, cinder, or scoria, is to be thrown in during the operation of melting or converting the iron, when by the usual routine course of procedure with puddling-furnaces, the said charge of crude, cast, or pig-iron begins to be melted so as to be capable of being stirred up and turned over with the iron tool which the workman uses for that purpose; then the application of my improvements is to commence by throwing upon the partially melted iron about two pounds weight at a time of powder of rich ore of iron, either with powder of charcoal or coal intermixed, or without, the iron being well stirred and turned over after every time that a dose of powder is so thrown upon it, in order that the powder may become intimately mixed with every part of the charge of iron. The fire should be kept up so that the heat of the iron will not become materially less than would be desired in the usual practice, notwithstanding the throwing on of the powder, and as soon as one dose of powder which has been thrown on and mixed up with the iron appears, together with the semi-fluid iron, to be at the full heat which the iron ought to have, according to the ordinary process, then another dose of about two pounds more of the same powder may be thrown on and mixed with the iron as before, and when the iron gets again to a full heat, another like dose may be added, and so on, always turning over and stirring the semi-fluid iron and intermixing each small dose of the powder as completely as possible with every part of the charge, and always keeping up the heat of the iron as near as can be to what would be done in the usual way of puddling, if my improvement were not used; in this way I add about as much powder of iron ore as will amount by weight to one-eleventh part of the weight of crude, or cast, or pig-iron with which the puddling-furnace is charged, or about forty pounds weight of such powder to the aforesaid charge of about four hundred and fifty pounds of iron, or in case powder of charcoal or coal is intermixed with the said powder of ore, then the weight of charcoal or coal will be at the rate of about one-sixteenth part of the weight of ore with which it is mixed, which will increase the said forty pounds of powder to about forty-two and a half pounds to a charge of about four hundred and fifty pounds of iron. The manner of adding the powder to the semi-fluid iron will be the same, whether it contain a mixture of charcoal or coal, or not, except that it may be found expedient to wet or damp the powder which contains a mixture of charcoal or coal with water before throwing it on; and note, such damping may be given when the powder does not contain a mixture of charcoal or coal, if the workman finds it desirable, all which is to be managed according to his experience in the working of the iron and its heat, in the same manner and with the same discretion as water is occasionally thrown into the puddling-furnace in the ordinary course of procedure. The usual fermentation of the iron will be increased, and its conversion into the malleable state, or coming into nature, will be facilitated by the addition of the powder according to my improvement, in manner aforesaid; and after that addition is made, the process is to be continued and concluded in the usual manner of puddling and making the iron into puddle-bars of common iron, without variation from the usual routine.

On account of my improvement I find that a ton or twenty hundred weight of puddle-bars can be made in the manner above explained by aid of my improvement from twenty one and a half hundred weight of crude, cast, or pig-iron, or from twenty and three quarters hundred weight thereof, when the powder of rich iron-ore is intermixed with powder of charcoal, as above explained. The quality of the puddle-bars so made being the same common iron as would be obtained by the usual mode of refining the crude, cast, or pig-iron, in the running-out-furnace or finery, and then puddling the refined metal or plate in the usual manner without aid of my improvement, but as that aid permits the preparatory refining operation to be dispensed with, a great saving of waste is attained, and although I have stated one eleventh of the weight of the charge of crude, cast, or pig-iron, as a proportion for the powder of rich iron-ore which may be used, the same may be varied with advantage in different cases, from one-eleventh to one-eighteenth, according to the nature and fusibility of the iron, and that if the rich ore which is used, the best proportion for every particular sort of iron and of ore, can only be determined by the discretion of the iron-master taking experience for his guide. To make bar-iron of a first rate quality by aid of my improvement, I take crude, cast, or pig-iron, of a richer quality than is usually employed for converting into malleable-iron, being such as is used by iron founders, and commonly called No. 2. grey iron foundry-metal, and which metal has been made without putting any slag, scoria, or cinders, into the blast-furnace. The mode of procedure is to be the same as last explained, putting a charge of about four hundred and fifty pounds of such No. 2, grey iron, into the puddling-furnace, and adding thereto about one-eighth part of its weight, or about fifty five pounds of powder of rich iron-ore, applying the same in successive small doses, in manner hereinbefore described, which will assist the fermentation and facilitate the conversion into malleable-iron. And I find that a ton or twenty hundred weight of puddle-bars of the first rate quality can be made in that way by aid of my improvement, from about twenty one hundred weight and three quarters of the said No. 2, crude, cast, or pig-iron, which is put into the puddling-furnace. Or from about twenty-one hundred weight thereof, if powder of charcoal is intermixed with the powder of rich iron-ore, the weight of powder of charcoal being about one-sixteenth of the weight of the powder of ore with which it is mixed, and although by aid of my improvement, the use of running-out fire or finery may be dispensed with, and Mr. Cort's original practice of puddling crude, cast, or pig-iron, as it comes from the blast-furnace, may, by aid of my improvement, be rendered fully available in practice; nevertheless my improvement is applicable with advantage to the puddling and conversion of refined metal or plate into bar-iron or malleable-iron. In such case I take such refined metal or plate as would be usually denominated under-blown, and which, in undergoing the operation of the running-out-fire or finery, will have lost in weight from eight to ten per cent. of the weight of the crude, or cast or pig iron, which was put into the running-out-fire or finery, and of such under-blown refined metal or plate I put into the puddling-furnace a charge of about four hundred and fifty pounds, and add thereto about one twentieth part of its weight or about twenty five pounds of powder of rich iron-ore applied in small successive doses in manner hereinbefore described, and I find that a ton or twenty hundred weight of puddle-bars can be made in that way from about twenty hundred and three quarters of the refined metal or plate which was put into the puddling-furnace, or from only twenty hundred weight thereof (or even less) when the said powder of rich iron-ore is intermixed with about one sixteenth part of its weight of powder of charcoal, in manner already explained; and the quality of the bar-iron thus made by aid of my improvement will be found equally fibrous as if it had been made in the common mode without any improvement. Or in applying my improvement, in case of puddling refined metal or plate, which has been fully blown by previous operation of the running-out-fire or finery, I put about four hundred and fifty pounds thereof into the puddling-furnace, and add about one-thirtieth part of its weight of powder of rich iron-ore, or about fifteen pounds thereof to the said charge, and in case such refined metal with such addition should not be found to melt so thin as is desirable for the proper working of the iron in the furnace, then from thirty to sixty pounds, or more, of grey melting iron may be mixed with the refined metal or plate, when the furnace is charged, and the said four hundred and fifty-pounds being made up by so mixing will have acquired more fusibility; and I find that a ton or twenty hundred weight of puddle bars can be thus made, by aid of my improvement, from about twenty hundred weight and three quarters of fully blown refined metal or plate, or from about twenty hundred weight and one-quarter thereof, when powder of charcoal, at the rate of about one-sixteenth part of the weight of the powder of iron-ore is intermixed therewith, as before explained; or for applying my improvement in case of puddling mixture of equal parts of fully blown refined metal or plate, and of crude, cast, or pig-iron which has not been subjected to the operation of the running-out-fire or finery, I put a charge of about four hundred and fifty pounds of such mixture into the puddling furnace, and add thereto about one-fifteenth of the weight of the charge, or thirty pounds of powder of rich iron-ore, and I find that a ton, or twenty hundred weight, of puddle-bars can be made in that way from about twenty-one hundred weight of the aforesaid mixture of iron, which was put into the puddling furnace, or from about twenty hundred weight thereof (or even less,) when powder of charcoal is mixed with the powder of ore in manner aforesaid; and although I have mentioned the proportions of powder of iron ore, with or without mixture of powder of charcoal, which I have found to answer best for performing my improvement with effect in several cases, which will be found to comprise the most usual circumstances of the manufacture of bar-iron, nevertheless those proportions must be varied to suit various circumstances, and as some guide to those variations, it may be observed that the more fusible the iron is which is put into the puddling-furnace, so a greater proportion of the powder of rich iron-ore may be used with advantage for the performance of my improvement, but if a greater quantity of such powder is added than is suitable to the fusibility of the iron, it will thicken too much and partially, so as to agglutinate into lumps which are unmanageable, and that should be avoided; and on the other hand, if the iron does not thicken so soon, or become converted into the malleable state as quickly as it might do, and as is desirable, or, as the workmen say, if it continues too long wet, then by adding more of the powder of such iron-ore it will thicken or become dry and ready for withdrawing from the furnace, the tendency of adding powder of iron-ore, according to my improvement, being in all cases to expedite the conversion of the iron into a malleable state in the puddling-furnace, and when powder of charcoal or coal is intermixed with powder of rich iron-ore, as hereinbefore stated of the powder of charcoal, being one-sixteenth of the weight of the powder of iron-ore, I have found to answer the purpose, but that proportion may be varied, and respecting the application of my improvement in cases of puddling crude, or cast, or pig-iron, which has been made by putting slag, cinder, or scoria along with ore or mine into the blast-furnace according to a practice before explained, although the same may be puddled without an absolute necessity for a previous preparation in the running-out-fire or finery, nevertheless I prefer and recommend that such pig, or crude, or cast iron, should be prepared by the operation of the running-out-fire or finery, and thereby blown or refined to such a degree as may be most suitable to its peculiar quality in the same manner as is now commonly practised with such metal, and then to charge the puddling-furnace with such refined metal, and to apply my improvement during the process of puddling the same, in the manner hereinbefore directed, for puddling refined metal or plate.

Having now described the nature of my improvement, and the manner in which the same is to be performed, I, the said David Mushet, do hereby declare that the new invention whereof the exclusive use is granted to me by the said letters patent, consists in adding rich native ore of iron in powder (either with or without intermixture of powder of charcoal or coal) to iron which is undergoing the process of puddling in the puddling-furnace, in order to be by that process converted into the malleable state, and manufactured into bar iron.—In witness whereof &c.

Enrolled April 22, 1836. Ibid.

Specification of the Patent granted to John Isaac Hawkins; *of the County of Middlesex, Civil Engineer, for an improvement in the Blowing Pipe for Blast-Furnaces and Forges.*—Sealed September 28, 1836.

The improvement consists in so forming the mouth or mouths of the blowing-pipe as to deliver the usual supply of air into the furnace or forge in a thin sheet or in thin sheets, possessing great surface in proportion to the bulk or volume of the stream of air, in order that the air may impinge upon and impart its oxygen immediately into a large quantity of the ignited fuel, the more advantageously to promote combustion, and also to avoid the comparatively chilling effect which is produced by the pouring in of cold air in a body of considerable thickness, as in the case in the ordinary mode of delivering it through a circular orifice, or through two, three, or even four circular orifices.

He adds, I sometimes enlarge the common circular orifice of the mouth piece or extremity of the blowing-pipe next to the fire to three or more times the usual diameter, and place a conical plug concentrically within the same, with the apex of the cone pointing towards the bellows or blowing machine, and the base of the cone lying even with the end of the mouth-piece towards the fire, or, lying a little within or a little without the end of the mouth-piece, the dimensions of the base of the cone being such as shall leave an annular aperture or air passage between the cone and the pipe, equal in area to the area of the ordinary mouth-piece or to the conjoint areas of both, or of all the mouth-pieces when two or more are used. By this arrangement the air is delivered in such a thin cylindrical sheet that its contained oxygen will find immediate access to the ignited fuel, and augment the intensity of the heat more advantageously than can be effected by a thick stream of cold air, or by two, three or four such streams; and I support and retain the cone in its place within the pipe or mouth-piece by means of two pins or bars passed horizontally through the sides of the pipe and through the sides of the cone, the pins or bars crossing at right angles the common axis of the cone and of the pipe; and I also place lateral guides to assist in keeping the cone concentric with the pipe, and I sometimes elongate the two holes in the cone, so that the cone may slide a short distance across the pins in the line of the common axis of the cone and of the pipe. upon being moved by a lever having its fulcrum in, or near, the side of the pipe, one arm of the lever projecting outward from the pipe, to which arm the power may be applied, and the other arm within the pipe reaching to the apex of the cone where it takes hold of a link affixed in any common manner to the apex of the cone. The lever, on being moved on its fulcrum by its outer arm lengthwise of the pipe, will cause the inner arm to move the cone in the line of the common axis of the cone and of the pipe. And I sometimes make the extremity of the mouth-piece surrounding the base of the cone a little conical, enlarging outwards towards the fire, whereby the sliding of the cone in the common axis will enlarge or diminish the area of the annular aperture, according as the sliding cone is carried towards or from the fire. And I sometimes carry the invention into effect by making the blowing-pipe with two or more branches, each branch terminating in a mouth-piece having a long narrow orifice, delivering the air into the furnace or forge, in a thin broad sheet, the conjoint areas of all the oblong orifices being equal to the area of the single circular orifice as commonly used. And in the case of a blast-furnace whether of the large kind for smelting of iron or of the smaller kind called a cupola in which cast-iron is melted. I distribute the mouth-pieces around the furnace so as to deliver the thin broad sheets of air into the fire at nearly equal distances. And in case of a forge I distribute the mouth-pieces at equal distances along the fire according to the length required to be heated, and I place the greatest dimensions of the orifices vertically and make them of such a height as shall the most effectually blow up a fire to the altitude required. And I sometimes make use of five or more mouth-pieces with circular orifices, but although they are much superior to a less number, I do not deem them so advantageous as the oblong form hereinbefore described.

Ibid.

NOTE. This patent was obtained for the inventor, JOHN BARKER, of Baltimore.

EDITOR.

Sugar:—Examined in its Commercial and Chemical Relations. By GEORGE GUYNNE and JAMES YOUNG.

(Continued from p. 180.)

We are inclined strongly to doubt the received opinion that it is the carbon of the charcoal which is the decolorating agent. That carbon alone has decolorating properties, appears certain from M. Bussy's elaborate paper (*Jour. de Pharm.* for June, 1822;) still we believe that the carbon takes away from raw sugar the aroma alone, and that it is the peroxide of iron and phospate of lime, which animal charcoal contains, that renders it useful in sugar refining. If some "grained" charcoal is burned to perfect whiteness, by destroying its carbon, and some raw sugar liquor is percolated through it, no decoloring takes place. This appears conclusive in favour of the carbon; nevertheless we would explain it in a different manner. Animal charcoal contains (besides other bodies not bearing on this question) carbon, peroxide of iron, phosphate of lime, and carbonate of lime. During the combustion in the open air, the carbonate of lime is decomposed, and converted into caustic lime, and part of the peroxide of iron into the protoxide of iron. Now as caustic lime forms soluble compounds with the vegetable foreign bodies in sugar, and, as its affinity is much greater than any of the oxides of the second class of metals, no decoloration takes place. But if the bone-ash is washed with diluted acetic acid, to neutralise the lime, and the acetate of lime formed thereby washed entirely away, the "bone-ash" dried to expel the water, and some raw liquor now percolated through it, considerable decoloration takes place; not quite so much certainly as before burning. This difference we believe is caused solely by the conversion of part of the peroxide into protoxide of iron; protoxide having no affinity whatever for either mucilage or gluten. These views receive confirmation from the following experiments. If three parts of hydrated peroxide of iron, and 97 parts of diphosphate of lime (neutral phosphate,) whilst still moist, are intimately blended together, placed on a water-bath, and perfectly dried, and then carefully pounded into grains and "raw liquor," percolated through, this composition will be found fully as powerful in decolorating as any animal charcoal obtained from bones.

If some of the carbonate of iron of the apothecary, which is well known to be a mixture of peroxide and carbonate of protoxide of iron, is diffused through water and some clean washed coarse grained sand is mixed with it, and the whole carefully dried at a boiling-water heat (212°,) so as to get the oxides to form a red coating round each grain of sand, and some raw liquor percolated through the "bed,"* it will prove to be a much stronger decolorating agent than animal charcoal. If the "liquor" is tested, it will not show a trace of iron. The action which takes place in this experiment is, the carbonate of protoxide of iron, owing to its very feeble affinity, is decomposed by the lime in the sugar, which is converted into the carbonate of lime, and the peroxide of iron instantly unites with the mucilage, &c. The oxide of iron, which the raw sugar originally contained, being totally insoluble in liquor, without the aid of either an acid, alkali, or alkaline earth; and in this case, it being lime which rendered it soluble, and that body being abstracted, the oxide of iron falls down insoluble also. We may remark here, that pure peroxide of iron is insoluble in liquor containing lime. If such liquor is boiled for some time on peroxide of iron, the peroxide is partially deoxidised, and becomes converted into protoxide, which is immediately dissolved. The oxide of iron in raw sugar, although minute in quantity, materially darkens the colour. The best mode of detecting its presence is by adding a few drops of solution of ferro-sesquicyanuret of potassium to some "raw liquor," contained in a test tube, shaking well, and then putting in a few drops of diluted sulphuric acid, and again shaking, the distinctive colour immediately appears. The sulphuric acid is necessary, because the ferro-sesquicyanuret of potassium does not indicate the iron unless the quantity is considerable; whereas, with the acid, the oxide of iron can be detected to the $\frac{1}{50000}$ part.

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* Percolating is an excellent means of forming saturated solutions of salts, like the permuriate of iron, which are troublesome to prepare by the usual mode. Dry the substance on the sand bath, and, if for the permuriate of iron, allow muriatic acid very slowly to filter through the salt till thoroughly saturated.

On filtering "raw liquor" through grained charcoal, we believe the action to be similar to that given of the carbonate of oxide of iron. The phosphate of lime unites with the lime contained in the liquor, and is converted into the triphosphate. The peroxide of iron unites with the mucilage, gluten, and tannic acid, the carbon absorbs the aroma, and the protoxide of iron is precipitated; the lime being abstracted by which it was held in solution in sugar.

That the phosphate of lime plays an important part in decolorating sugar, will receive strong confirmation from the effect it produces when mixed with that matter called the *residuum* of the Prussian blue makers. This charcoal, from its powerful decolorating properties, has been repeatedly tried in the sugar-house, but always abandoned from the injury it has been found to do the sugar. It is well known to contain a large quantity of oxide of iron; when mixed with raw liquor, the lime and sugar dissolve the oxide of iron, and here the injury takes place. Now, in order to prevent this, all that is required is to mix some diphosphate of lime with the *residuum*. The diphosphate abstracts the lime from the liquor, and when this takes place the oxide of iron is no longer soluble.

It may appear strange that as animal charcoal contains only a small quantity of peroxide of iron, the precipitation of mucilage, &c., can be due to this body, because it requires an immense quantity of charcoal to decolour "raw liquor" perfectly. This may be met with saying that all the impurities in 112 lbs. of raw sugar do not really weigh more than a few ounces, certainly not half a pound. The following will prove this to be true. Let a quantity of animal charcoal, in grains, be washed repeatedly with boiling water, in order to carry away the impure carbonate of ammonia it always contains when new. Let it now be heated to redness, in a retort, which will prevent access of air, and then allowed to cool completely without exposure; as soon as it is quite cold, it should be put into a suitable vessel, with a perforated false bottom, to the depth of 36 inches, and raw liquor allowed slowly to percolate through the bed of charcoal. The liquor which comes through, if tested, will be found perfectly free from mucilage, gluten, tannic acid, oxide of iron, and lime, and its crystallising power much increased. The only foreign bodies which it contains, are faint traces of acetate of lime and chloride of sodium. Let the sp. gr. of the "raw liquor," before pouring on the charcoal, be taken very accurately, along with the temperature; let this sp. gr. be, say 1.2800 temp. at 60°. If the sp. gr. of the liquor which first comes through the bed of charcoal be accurately taken, it will be found to be 1.2795 at 60°; any person with an accurate balance can be satisfied that this minute difference in the sp. gr. will not amount to more than a few ounces of foreign bodies in 112 lbs. of raw sugar.

It is the custom of those sugar houses, in which "grained charcoal" is used, to have it reburned in a retort again and again; but after a time the decolorating power of the charcoal becomes lessened, and this deterioration gradually increases until it arrives at a point where it is more economical to buy new charcoal than to reburn the old. This deterioration arises partly from the lime contained in the sugar being gradually deposited, and which cannot be got rid of by burning, slowly converting the bone phosphate into the triphosphate of lime. When this is accomplished throughout the entire charcoal, no farther abstraction of lime from the liquor takes place, and the grain thereby suffers injury. The "finings" which the sugar refiners use contain sulphate of potash, and this sulphate, as it slowly passes through the charcoal, becomes decomposed; the decomposition ending in sulphate of lime and phosphate of potash; the sulphate remains in the charcoal and gradually incrusts the peroxide of iron. The decomposition of the mucilage, &c., by burning, produces an increase of pure carbon, which also covers the peroxide of iron.

After a careful examination of the advantages and disadvantages attending the employment of grained charcoal, we believe that the *best* plan now in operation in Great Britain admits of considerable improvement. A simple experiment will prove that the refiner does not obtain all the *grain* raw sugar is capable of yielding by this plan. The "green sirups" produced in the sugar house from "raw sugar," which has passed through grained charcoal, yields the refiner a *poor article*, nothing at all approaching the quality of refined sugar. Let some new grained charcoal be carefully washed with hot water and dried; prepare a suitable vessel for filtering, and allow some green sirups of the above kind slowly to percolate through. If the first liquor which comes through the charcoal be boiled in a vacuum pan, and afterwards treated as for making refined sugar, an article will be produced equal in quality to the *double standard* of the London customs. Some of the refiners have attempted to do something like this, but they found that the quantity of charcoal required was so enormous, and they were so inundated with *sweet waters*, so as to make the ordinary method of disposing of the green sirups far preferable.

Of the foreign bodies existing in raw sugar which it is practicable to remove, three (mucilage, gluten, and tannic acid) are vegetable, and two, (oxide of iron and lime) are mineral bodies. It is evident, therefore that no one agent can remove bodies so dissimilar in properties.

Of the vegetable bodies, tannic acid alone has any thing approaching to a strong decomposing power. The affinities of mucilage and gluten are of the most feeble kind, incapable of neutralising either acids or alkalies. It is evident, therefore, that the mere change in the nature of mucilage and gluten will answer no useful purpose; they must be entirely removed from the sugar.

As lime forms soluble salts which are neutral to test paper, it may be converted into acetate of lime which does no injury to the sugar; not so the oxide of iron, for as all its soluble salts have an acid reaction on test paper, they must be precipitated.

In seeking for agents suitable for the above purposes, two essential circumstances should be borne in mind; viz., that they must be cheap, and that they do not injure the "grain" of the sugar. A failure in either of these conditions would be fatal to their employment. If two ounces of an article as expensive as nitrate of oxide of silver would remove every impurity from a cwt. of raw sugar, or a body such as chlorine, which injures the grain, would do the same, neither could be employed on the large scale.

Overlooking tannic acid, for the present, let us turn our attention to mucilage and gluten, which are soluble in acids; and as they must be entirely removed from the sugar, it is plain that the employment of any of that extensive class of chemical agents comprehended under the denomination of acids, and other bodies possessed of nearly similar properties (as chlorine, &c.,) are totally useless for this purpose, because their application would only produce a mere change in the nature of mucilage and gluten. These bodies being also soluble in alkalies and alkaline earths, and none of their salts having any power of precipitating mucilage and gluten, they are likewise inadmissible.

Several oxides, such as alumina, peroxide of iron, oxide of lead, &c., when in the hydrated state, will precipitate colouring matter from "raw liquor," and their powers in this respect are considerably increased by being mixed with bodies, such as diphosphate of lime or carbonate of oxide of iron, which have the power of abstracting the lime from the "liquor," but the quantity required of any hydrated oxide to remove the entire mucilage and gluten from sugar is so very great as totally to prohibit their employment for the above purpose. The following case will prove this. Animal charcoal, in fine powder, is much more powerful as a decolorating agent than alumina, peroxide of iron, &c. To effect the degree of improvement now obtained in the sugar houses (by percolation through grained charcoal,) would require 50 lbs. of finely powdered charcoal for 112 lbs. of raw sugar. This "fine" charcoal cannot have its strength restored in the ordinary way like the "grained" charcoal; it must be rejected as waste; besides this, the quantity of "*sweet waters*" produced by using so large a quantity would be ruinous to the sugar refiner. To show the difference still more characteristically, it will be found that if "grained" charcoal be employed in the same way as "fine" charcoal diffused through the liquor, by stirring, that one pound of "fine" will destroy more colour than four lbs. of "grained" charcoal. It has been found by experience that a greater proportion than 5 lbs. of fine charcoal to 112 lbs. of raw sugar cannot be employed advantageously. Now what experience has proved of "fine" charcoal, applies most strictly to metallic oxides. They cannot be used so as to make an improvement on the present process of sugar refining.

The salts of the second class of metals may be divided into soluble and insoluble salts. Where any of the insoluble salts, such as the disulphate (subsulphate) of peroxide of iron, dinitrate (subnitrate) of oxide of lead, &c. precipitate mucilage and gluten, it will be found that the same objections which prevented the employment of hydrated oxides prevail equally here.

The soluble salts may be divided into three classes; first, those which are composed of an equivalent of acid and base; second, those which have two or more equivalents of acid to one of base; third, those which have two or more equivalents of base to one of acid.

Vegetable principles possessed of affinities so feeble as mucilage and gluten are incapable of abstracting the base from the salts of the first or second class, without the aid of some alkali or alkaline earth; as, for instance, the tersulphate of peroxide of iron (persulphate of iron,) will not precipitate pure mucilage when dissolved in distilled water; but if the mucilage is dissolved in lime-water, immediate precipitation ensues if no excess of tersulphate has been employed. It has been shown, in an earlier part of this paper, that it is impossible to decompose a salt when in solution in "liquor," by the aid of an alkali or alkaline earth, without such decomposing body being in excess; now as alkalies, &c. injure the "grain" and "colour" of sugar, independent of the property they possess of dissolving mucilage and gluten, it follows that salts of the first and second classes can not be usefully employed as precipitators. Triacetate of oxide of lead (subacetate.) will be found the cheapest and most powerful precipitator of mucilage and gluten amongst the few salts constituting the third division; it also forms an insoluble compound with tannic acid.

Having given ample directions for its use in another place, it only remains for us to state, that its employment, according to our instructions, will greatly improve sugar refining as practised in this country, and prove of most essential service to the planter as a substitute for "temper," removing any impurity from cane juice. Believing that the period will arrive when triacetate of oxide of lead will be used for the purpose of removing vegetable impurities from sugar, we shall examine the mineral bodies (lime and oxide of iron) contained in raw sugar, along with the oxide of lead left in the liquor by its employment.

Acids, as oxalic, sulphuric, &c., which form insoluble compounds with lime and oxide of lead, cannot be used with advantage in the removal of the above bodies from "liquor," for an excess of acid must be employed for effecting this object; this excess will destroy the "grain," and it cannot be corrected by the aid of alkalies, &c., without such bodies also being in excess.

Acids can be neutralised or precipitated, when dissolved in liquor, by diffusing through such liquor suitable inert bodies, such as carbonate of lime, and carbonate of magnesia, without producing injury to the grain. But the objection which causes the rejection of the metallic oxides in practical sugar refining, prevails equally against the employment of carbonate of lime, &c., by diffusion, for neutralising acids existing in liquor. The only practical method of neutralising acids in liquor is, by allowing the liquor to percolate through a bed of suitable, insoluble material, as carbonate of lime, &c., in the manner described by us in our specification.

There are several salts, as sulphate of soda, ferrocyanuret of potassium (prussiate of potash,) oxalate of ammonia, &c., which will completely decompose the soluble salts of oxide of lead, when water is the medium in which they (salts of lead) are dissolved. If liquor has acetate of oxide of lead dissolved in it, and is itself not acid to test paper, no excess of sulphate of soda, ferrocyanuret of potassium, &c., will entirely remove the lead. If, however, the liquor is made slightly acid, or the sulphate of soda, &c. acidulated, the entire removal of the lead is effected.

We would explain the above anomaly as follows. In order to precipitate the mucilage, gluten, and tannic acid, from raw sugar, an excess of triacetate of oxide of lead must be employed. The sugar abstracts a part of the oxide from the excess, and forms a soluble saccharate of lead; the triacetate being thereby converted into the acetate. When, therefore, sulphate of soda, &c. are added to liquor in the above state, decomposition of the acetate of oxide of lead alone takes place; the saccharate of oxide of lead is not acted on. If now a little acetic acid be added, the saccharate is decomposed, and changed into the acetate, when it is precipitated by the sulphate of soda, &c.

We are aware that this explanation is contrary to the authority of Berzelius, who states "that saccharate of oxide of lead, is insoluble even in a large quantity of boiling water." However, the following experiment will prove that there is a soluble saccharate of oxide of lead even when the temperature does not exceed 60°. Take a glass tube, 36 inches long, and one inch in diameter, tie a piece of linen over one end, and fill three quarters of the tube with "flake litharge;" prepare some thin liquor by dissolving a pound of refined sugar in 12 pounds of cold water, pour the liquor into the tube, and if what comes through the litharge be tested, it will be found to contain a considerable portion of oxide of lead.

From the necessity of conjoining acids with sulphate of soda, ferrocyanuret of potassium, oxalate of ammonia, &c. it is plain no salts of the above kind can be employed more advantageously than sulphuric, oxalic acids, &c., for the purpose of removing oxide of lead from sugar.

Several of the super-salts, as binoxalate of ammonia, bisulphate of soda, biphosphate of lime, bitartrate of potash, &c., will precipitate oxide of lead from liqour, but the same objection which prevails against the employment of sulphate of soda is equally applicable.

The best agents for the precipitation of oxide of lead, lime, and oxide of iron from liquor, are the diphosphate of lime and neutral solution of phosphoric acid and soda, as recommended in our specification. The explanation we would give as to the action of the diphosphate of lime is, that that body gives off a portion of its acid to the lime contained in the liquor, forming diphosphate of lime, and thereby becomes partly converted into the triphosphate of lime; when the lime is abstracted, the oxide of iron falls down insoluble, or perhaps becomes converted into the diphosphate of oxide of iron; another portion of its phosphoric acid precipitates the saccharate of oxide of lead, forming the insoluble phosphate of oxide of lead. The remainder of the diphosphate exchanges with the acetate of oxide of lead, forming the insoluble phosphate of oxide of lead, and the soluble acetate of lime. A similar explanation attends the action of all the phosphates, including even the animal charcoal of the sugar-house. A very slight modification, however is observed when the soluble phosphates, as triphosphates of soda and basic water (rhombic phosphate of soda,) are substituted for the diphosphate of lime. In this case acetic acid is always set free. This arises from the tendency of phosphoric acid to form triphosphate of oxide of lead.

We will conclude our paper with the following practical advice. The planter and beet-root sugar manufacturer cannot be too much alive to the destructive effects which follow the employment of any alkali or alkaline earth to neutralise the acids existing in the juices of the beet-root and the cane. There is no possibility of neutralising an acid in saccharine solutions by the use of an alkali, &c., without such body being in excess.* The employment of "temper" is nothing but a choice of evils, even to the planter himself. To the refiner, in Europe, the mischief occasioned by its use is incalculable. Its effects can be traced throughout his entire operations. In the raw sugar it darkens the colour by the combination it forms with oxide of iron and sugar. It exerts a most pernicious effect on his grained charcoal, and it is the cause of the "smear" with which he is so frequently annoyed when working up his "poor goods." Br. Ann. Med.

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* The natural acids of the cane juice, &c., should be neutralised by percolation through grained carbonate of lime.

Progress of Civil Engineering.

Museum of Economic Geology.

The laws which, in several of the United States, provide for Geological surveys, do, we believe, very generally, make it the duty of the State Geologists and Naturalists, to provide specimens of the various rocks, minerals, &c., which claim their particular attention, and also designate the places where such collections shall be preserved. That Museums of this Character, may eventually furnish most important *material,* for the study of young Engineers, Architects, Surveyors, and Agriculturists, and, also, the means of illustration in Lectures to the public, on civil economy, and the best means of employing and managing the natural resources of the country, no one, we trust, will be disposed to doubt. There is, on this account, an obvious propriety in establishing at least two Museums of this nature in each State —One at the Capital, accessible to every member of the Legislature and other officers of the Government; and the other, in the most populous city or town in the State, where stated courses of Lectures may be given with the greatest advantage ;—and the time, it may be hoped, is not very remote when such lectures will be open, as in the public Schools of France, at a modicum of expense, to all suitable recipients of the knowledge thus attainable.

The following article indicates a spirit of this kind, in operation in Great Britain.

More than a twelvemonth has elapsed since we noticed the first institution of the above national museum, and although its growth, to be commensurate with its importance and extent, must necessarily be slow, we learn with pleasure, from a valued correspondent, that the first branch of it, established in Cornwall, is making rapid and satisfactory progress. The following account of the museum will be read with interest by many of our readers, and we look forward to the time when a similar collection will be formed in the coal and iron districts of our central and northern counties:— G.

"The Museum of Economic Geology was established by his Majesty's Government, under the department of Woods and Works, in consequence of the representation of Mr. De la Beche, that during the progress of the Ordnance Geological Survey, there would be abundant opportunities of procuring specimens illustrative of the mineral wealth of Great Britain, and of the application of geology to the useful purposes of life generally. It is intended that the museum shall be divided into three or four sections. One to contain a series of woods, collected during the progress of the geological survey, to illustrate the connexion of the geological structure of different districts with their agricultural character. This section will also show the best road materials that can be obtained in the different districts and their localities, and will also, in connexion with the Ordnance Geological Maps, show the kind of country likely to be traversed in any proposed lines of canals or roads, and thus be useful in the formation of estimates for such works.

"A second section will illustrate British mining, containing specimens of the mineral veins, coal beds, &c. of different districts, with such others as illustrate their geological mode of occurrence. We understand that, from the very liberal spirit exhibited by the gentry and mining interests of Cornwall, where the geological survey is at present in progress, this part of the collection is already rich, as respects the lodes of Cornwall, the museum having received from that county donations of the most valuable kind —indeed, such as could not have been procured without the liberal co-operation above noticed. Models of mining machinery are now in preparation to accompany this section of the museum, and there will also be others illustrative of the working of lodes and coal-beds, and of their general mode of occurrence.

"A third section will contain those British rocks which either have been, or advantageously may be, employed in architecture or public works. Among the specimens already collected, we understand there are some porphyries (elvans) of Cornwall of great beauty, and which, from their abundant occurrence, might be extensively employed for ornamental purposes. We believe, however, they have hitherto received little attention, except from Joseph Austen, Esq., who is now causing fine slabs of Withiel porphyry and schorl rock, from the neighborhood of St. Austel, to be cut and polished by machinery for the decoration of his house at Place, Fowey. We are also informed that there are many specimens of the Lizard Serpentine, which show that this beautiful material may advantageously be employed for a variety of ornamental purposes, at comparatively small cost.

"A fourth section will show the various British mineral substances which either have or may be employed as pigments, and which can be used for ornamental purposes, on a small scale, for bijouterie, &c.

"The museum is too much in its infancy to be open to the public at present, and the collections are merely arranged with regard to temporary convenience, but we have little doubt that, when more advanced, this establishment will be found to be one of great public utility for a variety of purposes." Min. Jour.

Materials for Mortars and Cements.

The study of the nature of different species of mortars, demonstrates that their solidification depends on the formation of silicate of lime, and sometimes also of silicate of alumine. These silicates retain some water, and assume the firmness of stone, whilst the hydrate of lime in excess unites by degrees with carbonic acid; and consequently, solidified mortar may be considered a compound of carbonate of lime and of a zeolite. *Opal*, *pumice stone*, *obsidian* and *pitch stone*, pulverized, form with hydrate of lime a good cement. But only the surface of each grain of *quartz* or *sand*, is transformed into a hydrated silicate; and though this is sufficient to unite the mass, solidification does not take place so promptly. The mass becomes the more solid, the more finely the quartz is pulverized. If the pulverized quartz be mingled with one-fourth part of lime, and, after thoroughly calcining the mixture, it be pulverised and mixed with one-fifth part of lime, it forms a hydraulic cement which becomes so hard as to be susceptible of a polish. Feldspar hardens slowly, and with lime only after five months; but if calcined with a little lime it is much improved. Common potters' clay, which is absolutely worthless in its natural state, affords with lime, when calcined, provided it contains but little iron, a cement which readily hardens.

Steatite, which has been subjected to a red heat, cannot be made to unite with lime, and thence I concluded that magnesia has a strong affinity for silicic acid, attempted to employ calcined dolomite, in the place of ordinary lime, and found that it surpassed it, both as a material for ordinary mortar, and also for a hydraulic cement. I obtained a good mortar with this last material and calcined marl.——Fuchs. *Bérzelius' Jahrsbericht.*

Mag. Pop. Sci.

Progress of Physical Science.

Instructions for making and Registering Meteorological Observations.

Continued from p. 197.

"*Of the Temperature of the Sea.*—The surface temperature of the water at sea should be registered, as a matter of course, with the same regularity and at the same hours as the barometer and thermometer. It is more conveniently (and with quite accuracy enough for the purpose) obtained by taking up a bucketful of the water and stirring round the thermometer in it. Whenever a change to the extent of 2° Fahr. appears to have taken place since the last observation, a fresh bucketful should be taken up, and the observation repeated. It should also be noticed whether rain has fallen since the last observation. A sudden depression of 3° or 4° indicates the near approach of land. In a voyage lately made from England to the Cape of Good Hope, the temperature of the surface water fell at once 9° Fahr., on approaching within a few miles of the entrance of Table Bay.

"The temperature of the sea at considerable depths can hardly be regarded as a subject of ordinary meteorological inquiry and regular registry, though undoubtedly one of much physical interest; for which reason it is not considered necessary to dwell further on it.

"*Of the Hygrometer, &c.*--In the absence of Daniell's Hygrometer, or of ether to cool it, the degree of dryness of the air may be ascertained by observing the temperatures marked by two thermometers suspended freely side by side (but not in contact) in the shade, and completely defended from all radiation *to* or *from* the *sky*, the one having its bulb and stem naked, the other with the bulb and lower part of the stem wrapped in linen or cotton, and thoroughly wetted with pure spring or rain water. The temperatures indicated by both should be noted when the wetted thermometer refuses to sink lower, and the conclusions left for subsequent calculations. The naked thermometer may be the 'External Thermometer' itself, in which case a coated thermometer may be kept always suspended near it, completely screened, as above mentioned, and wetted some minutes previous to the regular daily readings.

"If a hair hygrometer be used, its points of absolute moisture and dryness should be frequently ascertained, as they are apt to change. The former may be found by keeping it some time in a close covered jar lined with wet blotting paper, and having water in it, and noting the point of moisture beyond which it refuses to go. The latter, by keeping it in the same manner in a jar perfectly air-tight, over fresh burnt quick-lime, till it refuses to indicate a higher degree of dryness.

"The best measure of the *momentary evaporating power* of the air seems to be the depression of the wetted thermometer below the dry one. But the *actual evaporation* from a given surface is quite another thing, and a question may very reasonably be raised, how far any useful approximation to a knowledge of the total evaporation from an extensive and diversified surface, unequally moistened, and variously exposed to the sun, defended by clouds, or refreshed by dews, *can* be obtained by any small or local experiments.

"The Rain-gauge is an instrument of such extremely easy construction, that any person who lives near a tinman can procure one. In arid climates, it must, however, be remembered, that it will often need examination and cleansing, owing to long intervals of disuse in which insects and dust may lodge in it. It will often happen, too, that the slight train of one day, if left unregistered, may be entirely lost by evaporation in the next,—nay, that slight and transient showers may never enter it, being evaporated from it as they fall. The effect of copious dew, too, must be separated from that of rain, so that the mere registry of the contents of the gauge is not of itselt a sufficient indication whether rain has fallen in the night or not. However, there are usually good reasons for decision on this point from other indications. Attention to the amount of dew is very necessary, not only because the meteorological questions involved are of a high degree of interest generally, but because in arid climates the dews are of almost as much importance to the maintenance of vegetation as the rain.

"In stating the quantity of rain daily received in the gauge, the height of the receivei above the soil should be mentioned, experience having shown that the quantities of rain which actually fall on a given area on the ground, and at a very moderate height above it, often differ materially. In some localities and circumstances, the rain-drops receive accession from the air as they descend, in others they undergo partial evaporation. The former is generally the case in cool moist climates,—the latter may be expected in this country.

"*Of the Wind.*—The points most important to remark respecting the wind, are,—

"1st. Its average intensity and general direction during the several portions of the day devoted to observation and registry.

"2ndly. The hours of the day or night, when it commences to blow from a calm, or subsides into one from a breeze.

"3rdly. The hours at which any remarkable changes of its direction take place.

"4thly. The course which it takes in veering, and the quarter in which it ultimately settles.

"5thly. The usual course of *periodical winds*, or such as remarkably prevail during certain seasons, with the law of their diurnal progress, both as to direction and intensity—at what hours, and by what degrees, they commence, attain their maximum, and subside, and through what points of the compass they run in so doing.

6thly. The existence of crossing currents at different heights in the atmosphere, as indicated by the course of the clouds in different strata. In observing these, it is advisable to fix the eye by some immovable object, as some point of a tree or building, the sun, or the moon; otherwise mistakes are apt to arise.

"7thly. The times of setting-in of remarkable hot or cold winds,—the quarters from which they come, and their courses, as connected with the progressive changes in their temperature.

"8thly. The connexion of rainy, cloudy, or fair weather, with the quarter from which the wind blows, or has blown for some time previous.

"9thly. The usual character of the winds as to moisture or dryness, not as deduced from mere opinion or vague estimation, but from actual observation of the hygrometric state of the atmosphere during their prevalence.

"Among these particulars it will be seen that some are of a nature susceptible of daily observation and registry, while others call for an exercise of the combining and inductive faculty on the observer's part, and cannot be made out otherwise than by continued attention and habitual notice of phenomena, with a view to the investigation of their laws. The general impression left upon the mind as to any of the points of this kind above enumerated, by the occurrences of the past month, will therefore be more properly stated, in the way of summary remarks at the end of monthly registers, than as entries under particular days.

"*Of the State of the Sky.*—In describing the state of the sky as to clouds, &c. the observer will bear in mind that it is only in that region of the sky which is vertically above him that the true forms and outlines of the clouds are exhibited, and the area they cover, as well as the intervals between them distinctly seen. As they approach the horizon in any direction, their extent is foreshortened by perspective, their apparent magnitude diminished by distance, and their intervals covered in, and hidden by their mutual interposition. In estimating, therefore, the quantity of clouds in the sky, regard must be had to this, and our judgment should rather be formed on a view of the region extending from the zenith every way half way down to the horizon, than from the aspect of the heavens below that limit. It would be better to notice both, and state, separately, the proportions in which each are covered, and the quarter of the horizon towards which the chief masses in the lower region lie.

"The general aspect of clouds, as classed under the heads Cumulus, Cirrus, Stratus, &c., should be noticed, and especially the height of this inferior surface, or the level of the *vapour-plane*, should be estimated. In a mountainous region this is easy, so long as the vapour-plane is below, or not far above, the summits of the hills, and in such regions the formation and dissipation of clouds in the neighborhood of the mountain-summits, under the influence of certain winds, form a subject of study of a highly curious and interesting nature.

"The formation of clouds at night, during calm weather, under the influence of a gradually descending temperature, is another point worthy of attention. It frequently happens that, without any perceptible wind, the sky will suddenly become hazy in some one point, and the haze condensing and spreading, in all directions, without a wind, the whole heaven will become overcast in a remarkably short time. The same thing will sometimes occur nearly at the same hour for many nights in succession. Such phenomena should be noted whenever they occur.

"*Of Thunder and Lightning, and of the Electrical state of the Air.*—Connected with this part of the subject is the observation of shooting-stars and luminous meteors. Remarkable ones should be noticed, and the moment of their appearance, their direction, duration, length of path, and *course among the stars*, ascertained and noted, with the phenomena of their increase and decay of light, apparent size, separation into parts, trains left behind, &c. The *general* direction (if any) which they observe on particular nights, is a point also to be attended to. When these splendid phenomena occur frequently and brilliantly in clear skies, there can be no doubt of their affording an available method of ascertaining the differences of longitude of the most distant stations, if duly observed by persons furnished with means of ascertaining the time.

"Thunder-storms, of course, will be noticed when they occur, under the general head of the weather, but it is of consequence also to notice distant lightning, not accompanied with thunder audible at the place of observation (by reason of its great distance,*) especially if it takes place many days in succession, and to note the quarter of the horizon where it appears, and the extent it embraces. In an actual thunder-storm, especial notice should be taken of the quantity of rain that falls, and of the fits or intermittences of its fall, as corresponding, or not, to great bursts of lightning, as also of the direction of the wind, and the apparent progress of the storm with or against it.

"Observations of the electrical state of the air in serene weather are unfortunately too much neglected. The apparatus they require is simple, and by no means costly, and may be constructed, indeed, by any one for himself with ease.

"Attention to the accumulation of regular and daily observations of a definite and numerical character is, in the first instance, of the highest importance. In order that such observations may be registered with uniformity and ease, printed skeleton-forms should be prepared and distributed to all persons who may be willing to undertake their filling up. These may comprise only the registers of the barometer and its attached thermometer, with that of the external thermometer, and a column of remarks for wind and weather, as being the most essential and indispensable elements of meteorology; but it is in the power of any one who pleases to supply additional information, and to those who have leisure, instruments, and inclination for the task, it is particularly recommended to observe regularly the wet thermometer, the self-registering thermometer, and weekly or monthly thermometers buried at different and progressive depths beneath the surface of the soil.

"The printed forms should provide for the arithmetical convenience of casting up the *means* for each month. In doing so, it is requested that care may be taken to verify the results by repetition, and (that usual sources of error may not escape notice;) it is recommended in every instance, before adding up the columns, to look down each to see that no obvious error of entry (as of an inch in the barometer, a very common error, or, what is more difficult of detection, an error in the first decimal place,) shall remain to vitiate the mean result. It is perhaps unnecessary to do more than mention the precaution of *counting* the days in *each* column on which observations occur, so as to admit of no mistake in the *divisor*, and to use throughout the decimal arithmetic in calculating the mean results. Care and exactness in these points will, in most cases, add greatly to the value of the registers, as it will be quite impracticable for any person, should observations accumulate in masses, unreduced or erroneously reduced, to undertake the overwhelming task of recomputing them.

"Although not, strictly speaking, a branch of meteorology, yet, as observations of the tides are now earnestly desired in every part that they visit,

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*"Thunder can scarcely ever be heard more than 20 or 30 miles from the flash which produces it. Lightning, on the other hand, may be seen (or at least its reflection on the clouds, forming what is called *sheet lightning*,) at the distance of 150 or 200 miles."

it would be especially desirable to obtain regular observations of the time and height of high and low water on the coasts, &c., according to the rules, and on the plan, proposed by Mr. Whewell, in his late researches on this subject." Mag. Pop. Sci.

On the Stony Matters which are employed in China in the time of Famine, under the name of Flour of Stone. By M. Biot.

The details which were communicated to the Academy of Sciences by M. de Humboldt concerning the existence of a stony substance, which is sometimes employed in Lapland, in the time of dearth, have recalled to my recollection the notice of a similar fact which has lately reached us from China, and which was reported in the correspondence of the missionaries. My son having likewise found the same fact, attested at many different periods in the Japanese *Encyclopédia*, with the dates attached, I requested him to translate the passages which related to the subject; and it has occurred to me that the Academy would regard with interest the collection of these documents concerning the employment of the article in a way much more general than we are usually led to believe.

The Japanese Encyclopedia, book lxi., "Upon Stones and Minerals," contains an article entitled *Chi Mien* or *Stone-flour*, of which we now present a translation; and in which it will be seen the same superstitious ideas prevail which M. de Humboldt had remarked in Laponia. "The *Pen-tsao-Kang-mou*"* remarks, "The flour of stone is not an ordinary substance, but a miraculous production. Many declare that it is produced in the time of famine. Under the Emperor *Hien-Tsong*, of the dynasty of *Tang*, in the period of *Tien-pao*, the third year (answering to A. D. 744,) a miraculous spring issued from the earth, and stones were decomposed and converted into flour." To the letter-press of this extract is conjoined a wood-cut, which represents the spring issuing forth in cascades, and the stones breaking up into slender threads, but these are so incorrectly indicated that it is not possible to draw any mineralogical inferences.

We subjoin some additional notices. "Under the Emperor of *Hian-Tsong*, of the same dynasty, in the period *Yuen-ho*, fourth year (A. D. 809,) stones were decomposed and became meal." Under the Emperor *Tching-Tsong*, of the dynasty of *Soung*, in the period *Tsiang-fou*, fifth year (A. D. 1012,) "a marrow was produced from stones which resembled flour." Under *Jin-Tsong*, in the period *Kia-yeou*, seventh year (A. D. 1062,) "the flour of stone was produced." Under *Tchi-Tsong*, in the period *Yuen-fong*, third year (A. D. 1080,) "stones were decomposed and became flour. All these kinds of flour were collected and eaten by the poor."

We now add the statement, made in 1834, by one of the Chinese missionaries, M. Mathieu-Ly, who is established in the province of *Kiang-Si*.† The facts which he describes relate to the same year 1834, and to the three preceding, so that they coincide with those mentioned by M. Retzius regarding Laponia. "Many of our converts will assuredly die this year from want; and it is God alone who can provide a remedy for so many and such aggravated necessities; all the crops have again been carried away by the overflowing of the rivers. For a period of three years now, an immense number of persons have supported themselves upon the bark of a tree which is found in the country; whilst others eat a light earth of a white colour, which has been discovered in a mountain. The earth can only be bought with silver, and it is not every one that can procure it. These wretched people first sold their wives, their sons and daughters, they then sold their tools, and the furniture of their houses; and even these they have finally demolished that they might sell the timber-work. Many of these unfortunate people were really rich four years ago."

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* This work is a collection of Chinese Natural History, compiled about A. D. 1575, from treatises which were still more ancient. M. S. Julien having kindly communicated to my son his copy of the *Pen-tsao-Kang-mou*, the quotation given in the Japanese Encyclopedia has been compared with the original text, and found to be accurate. Many of the places named are situated in the Northern Province called *Chan-Si*, where, the cold is often severe during the winter; others belong to the maritime province of *Chan-tong* and *Kiang-Nan*, near the mouth of the Yellow River. The provinces of *Hou-Kouang* and *Kiang-Si*, concerning which the missionaries attest the same fact, are different from these, and are situated in the valley of the Blue River.

† See Annales de la propogation de la Foi, No. xlviii. p. 85, Sept. 1836.

Another missionary, M. Rameaux,* writing concerning the province of *Hon-Kouang*, about the middle of the year 1834, supplies details which are not less deplorable. "The district *Fan-Hien*, he remarks, contained about a thousand converts; but their number has been exceedingly reduced by famine. A great number have come to me to demand the last sacraments. They calculate their resources, and accurately know, almost to an hour, the number of days they can subsist. They receive the sacrament of extreme unction when their means are exhausted, and then having nothing to eat, they calmly wait the moment of their demise."

Clearly to apprehend the cause of these calamities, and their frequent returns among an industrious society, which is chiefly agricultural, and has had the blessing of a steady government for a long course of ages, it is necessary to recollect that many provinces of China, more extensive than the half of the whole kingdom of France, are great uniform plains, traversed by immense rivers, whose beds are ever and anon choked up by the deposits which are left by the waters, so that it is necessary constantly to confine them by high dikes, which are maintained with immense labour. The provinces of *Hou-Kouang* and of *Kiang-Si*, for example, which have now been named, are thus traversed by the Blue and other great rivers. These circumstances afford every facility for irrigation, develop an agriculture in which industry is pushed very nearly to its limit, whereby the most abundant harvests are produced, especially of rice, which is cultivated even up the slopes of the hills, the water being forced up by hand-engines. So long as this state of things continues, the necessary result is an immense production of the means of subsistence, which leads to a corresponding development of the population. But if once the waters so far increase as to run over the dikes, they spread over the plain, inundate it, and swallow up a portion of the population: whilst those who escape the disaster, finding themselves ruined, and deprived of all their resources so long as the waters cover the soil, remain a prey to all the miseries which the missionaries describe, and, finally, in immense numbers, actually perish from hunger. This cause, conjoined with the awful catastrophes produced by earthquakes, which seem to be more frequent, more violent, and especially more widely spread in China than in most other regions of the globe, enables us, in a great degree, to understand the sudden vicissitudes which, as the history of China attests, so often occur in the number of the population of this vast empire; vicissitudes whose proportionate number bears no relation to the regular law of European population, as may be seen in a memoir inserted in the *Journal de la Société Asiatique.** Ed. New Philos. Jour.

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* Ann. de la propogation de la Foi, No. xlviii, p. 61.

On the Organic remains in the Coal Formation at Wardie, Scotland. By ROBT. PATERSON, M. D.

It is now almost universally admitted, that coal has resulted from the distribution of vegetable remains over areas of greater or less extent upon a previously deposited surface of sand, or argillaceous silt, and that after the entombment of this mass of vegetable matter, other layers of mud and clays were deposited on it; and this operation must have continued for a considerable length of time, during which it is more than probable that an abundant vegetation existed at no great distance from the spot where this process was taking place.

These changes are most likely to have gone on in estuaries or indentations of the land, into which this mass of vegetable matter was brought by rivers, while the sea made occasional, or in some instances continuous, incursions. In this manner we can easily see how, by accidental circumstances, the productions of the land or sea in greater or less abundance are occasionally met with in the strata of the carboniferous series, and how in one coal district an examination of its organic remains may afford one individual reason to believe, that the strata were chiefly deposited in salt water; a second, from the examination of a different locality, that they are entirely fluviatile, or probably he may imagine them lacustrine; while a third may see the same organic remains which have given rise to the two former opinions so mixed up and incorporated together in another place, as to leave no doubt on his mind that the two former cases were only extreme degrees of what he had noticed. Ibid.

"*An account of a Well at Beaumont Green in the County of Hereford fifteen miles from London, about a mile to the west of the road to Ware;*" *by* J. MITCHELL, LL. D., F. G. S.

This well was dug in 1833, in the premises of Mr. Munt, a magistrate of the county, and the information respecting it obtained from two gentlemen accustomed to collect evidence with the strictest scrutiny.

The strata passed through, were one foot vegetable mould, 15 feet gravel, one foot sand with flints, 83 feet gravel clay, 15 feet blue sand with black pebbles, 10 feet blue clay, 1½ feet fine soft white sand, or 126½ feet down to the chalk, which was penetrated for 40 feet, when a spring was met with; but the digging continued 17 feet lower to form a reservoir of water, and this was favoured by making the excavation in the chalk of a bell shape, but above this the well was 4½ feet in diameter.

When the well was dug the weather was dry, but on this becoming very wet the 15 feet stratum of blue sand and black pebbles began to emit foul air, by which one of the well-diggers was suffocated in descending. A hawk flying over the well fell into it, and a similar fate befell smaller birds, also wasps, bees and flies. On closing up the mouth of the well, with the exception of an orifice an inch in diameter, so powerful was the force of the issuing current of foul air that it raised a weight of twelve ounces of lead.

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* Memoire sur la population de la Chine et ses Variations, depuis l'an. 2400 avant. l'ère Chrètienne, jusqua'au 13e Siècle après; par Edouard Biot.

In fine weather there was on the contrary a strong draft down into the well.

Eight months afterwards the well was again entered when the stratum of blue sand and black pebbles appeared forming into plumb-puddling stone. The well was rendered safe by bricking it down to the chalk, and applying a thick coating of compost over the whole. Dr. Mitchell explains the phenomena by observing that the foul air was no doubt sulphuretted hydrogen, produced by the decomposition of water and iron pyrites. After long-continued rain, water penetrating into the bed dislodged the gas accumulated in the interstices where it was formed; while after dry weather had continued for some time, the openings produced in this bed on drying up would draw for a short time a supply of air to fill up the vacuities, and hence the draft observed to pass down into the well. The whole of the neighbouring district, to the extent of four miles, is called by the well-diggers, foul country. Similar phenomena were observed in digging a well on the opposite hill at Applebury, and also in forming some wells in the immediate vicinity of London. Ibid.

Mechanics' Register.

Plumbago and Black Lead Pencils.

There is only one purpose to which this form of carbon is applied in the solid state, viz., for the manufacture of black lead Pencils, and its adaptation to this end depends on its softness. In the state of a powder, plumbago is used to relieve friction. Its power in this way may be illustrated by rubbing a button first on a plain board, five or six times, and applying it to a bit of phosphorus, the latter will immediately burn. When rubbed on a surface covered with plumbago, double or triple the friction will be required to produce the same effect. One of the most remarkable circumstances connected with plumbago, is the mode in which it is sold. Once a year the mine at Borrowdale is opened, and a sufficient quantity of plumbago is extracted, to supply the market during the ensuing year. It is then closed up, and the product is carried in small fragments of about three or four inches long, to London, where it is exposed to sale, at the black lead market, which is held on the first Monday of every month, at a public-house, in Essex-street, Strand. The buyers, who amount to about seven or eight, examine every piece with a sharp instrument, to ascertain its hardness—those which are too soft being rejected. The individual who has the first choice pays 45s. per pound, the others 30s. But as there is no addition made to the first quantity in the market, during the course of the year, the residual portions are examined over and over again, until they are exhausted. The annual amount of sale is about 3000*l.* There are three kinds of pencils, common, ever-pointed, and plummets. The latter are composed of one-third sulphuret of antimony, and two-thirds plumbago. The first part of the process is sawing out the cedar into long planks, and then into what are technically termed tops and bottoms. The second, sawing out the grooves by means of a fly-wheel. The third scraping the lead on a stone, having been previously made into thin slices, to suit the groove: introducing it into the groove, and scratching the side with a sharp-pointed instrument, so as to break it off exactly above the groove. The fourth, glueing the tops and bottoms together, and turning the cedar cases in a gauge. The *ever-pointed* pencils are first cut into thin slabs, then into square pieces, by means of a steel gauge. They are then passed through three small holes, armed with rubies, which last about three or four days. Steel does not last above as many hours. Six of these ever-pointed pencils may be had for 2s. 6d. If they are cheaper than this, we may be sure that they are adulterated. In Paris, when you buy a sheet of paper in a stationer's shop, some of those pencils are added to the purchase. Now, these are formed of a mixture of plumbago, fuller's earth, and vermicelli. Genuine cedar pencils must cost 6d. each. If they are sold at a lower price they must be formed from a mixture, not from pure plumbago. Pencils are, however, sold as low as 4½d. a dozen. There is no patent which has been more infringed on than that of Mordan's, for ever-pointed pencils. Birmingham is the source of this infringement, where they are sold as low as ¾d. each, formed of composition. A thousand persons are now engaged in the manufacture of these pencils and pencil-cases. These facts were stated by Dr. Faraday, at the Royal Institution, April 22.—*Arcana of Science.*

Min. Jour.

Edge Rails.

The edge rail has now entirely superseded the tram or train-plate, and is used in all railways worked by locomotive engines. This form of rail causes much less friction to the wheels, there being considerably less surface of the former coming in contact with the latter. These rails are made of wrought iron, in 15-feet lengths, with an upper flanch slightly curving from the middle to the sides; the weight of rail is regulated by the tonnage passing along the line. On the Stockton and Darlington, Manchester and Liverpool, and Leeds and Selby Railways, the weight per lineal yard of the original rails is 35 lbs. On the Manchester Railway, however, rails of 75lbs. are being substituted. The size of the 35lb. rail is 3½ inches in depth, 2¼ inches as to the width of the upper flanch, and the upright portion three-quarters of an inch thick. The original rails of the London and Birmingham Railway, are 50 lbs. in weight, as also those of the Greenwich Railway; but on the former line 75lb. rails are being laid down, to a considerable extent. The Manchester and Bolton rail is 50lb. to the yard, but differs in shape from those used on every other railway. The top flanch is 2¼ inches wide, the bottom flanch 4⅝ inches wide, the depth $2\frac{6}{10}$ inches, and the thickness of the middle portion three quarters of an inch.—*Whishaw's Analysis of Railways.*

Min. Jour.

Steel Letter Press Engraving.

A young artist of Vienna, named von Sieglander, has, we are informed by the German papers, recently discovered a process by which he can execute engravings in the style of copperplate, which can, nevertheless, be printed with letter-press in the same manner as wood cuts. This invention may prove of some importance. The Hamburgh Correspondent tells us with as much solemnity as it speaks of the invention, that Mr. Ficker, Professor of Asthetics, at the High School of Vienna, has already given it the name of *Chalcoxlyography.* Mec. Mag.

LUNAR OCCULTATIONS FOR PHILADELPHIA, DECEMBER 1837.

Angles reckoned to the right or westward round the circle, as seen in an inverting telescope. ☞ For direct vision add 180° ☜

Day.	H'r.	Min.	Phase	Star's name.	Mag.	from Moon's North point.	from Moon's Vertex.
2	6	2	Im.	33 Capricorni	6,	130	150
2	7	11	Em.			293	327
5	4	43	Im.	9 Piscium	5,	103	71
5	5	50	Em.			335	315
12	14	56	Im.	c, Aurigæ	6,	125	182
12	15	50	Em.			214	272
17	16	43	Im.	*l*, Leonis	6,	350	345
17	17	26	Em.			290	302
24	20	45	Im.	α Scorpii	1,	73	57
24	22	4	Em.			244	243

Meteorological Observations for July, 1837.

Moon.	Days.	Therm.		Barometer.		Wind.		Water fallen in rain.	State of the weather, and Remarks.
		Sun rise.	2 P.M.	Sun rise.	2 P.M.	Direction.	Force.		
				Inches	Inches			Inches.	
	1	70°	86°	29.70	29.70	W.	Moderate.		Lightly cloudy—do. do. aur. bor.
⊗	2	64	78	80	85	NW.	do.		Clear—do.
	3	64	76	80	70	W.	do.		Partially cloudy—do. do.
	4	66	78	80	80	W.	Brisk.		Clear—do.
	5	66	81	70	70	W.	Moderate.	1. 5	Cloudy—shower.
	6	64	82	50	60	E W	do.		Cloudy—flying clouds.
	7	67	71	80	83	W.	do.		Cloudy—do.
	8	62	68	80	80	S.	do.	.45	Rain—cloudy.
☾	9	60	75	90	30.00	NW.	do.		Clear—do.
	10	58	76	30 00	29.95	W.	do.		Clear—do.
	11	66	76	29.85	80	W.	do.	.15	Cloudy—shower.
	12	62	74	80	85	E	Brisk.		Cloudy—flying clouds.
	13	59	78	95	30 03	E.SE.	Moderate		Clear—do.
	14	61	82	85	29.75	W.	do.		Clear—do.
	15	66	84	70	70	W.	Brisk.		Clear—do.
	16	63	78	76	85	W.	Moderate		Lightly cloudy—clear.
☉	17	64	80	30.10	30.10	NW.	do.		Clear—do.
	18	66	80	10	5	S.	do.		Cloudy—clear.
	19	66	78	29.90	29 86	S.	do.	.2	Lightly cloudy—do. do.—shower.
	20	68	82	80	80	W.	do.		Clear—Lightly cloudy.
	21	64	80	90	90	W.	do.		Clear—do
	22	62	80	95	95	W.	do.		Clear—do.
	23	68	82	80	80	S.	do.	.2	Cloudy—do.—shower.
☽	24	72	84	65	60	SW.	do.		Lightly cloudy—do. do.
	25	61	76	74	65	W.	Brisk.		Partially cloudy—do. do.
	26	60	67	65	65	W.	Moderate	.45	Clear—rain.
	27	60	77	60	64	W.	Brisk.		Clear—do.
	28	63	82	75	80	W.	Moderate		Clear—do.
	29	68	84	80	76	W.	do.		Partially cloudy—clear.
	30	65	78	85	85	SE.	do.		Cloudy—do.
	31	69	80	80	80	SE.W.	do.		Cloudy—lightly cloudy.
	Mean	64.35	78.48	29 81	29.81			2.14	

	Thermometer.	Barometer.
Maximum height during the month	86.00 on 1st.	30.10 on 17th 18th.
Minimum do.	58.40 on 10th	29.50 on 6th
Mean do.	71.42	29.81.

JOURNAL

OF THE

FRANKLIN INSTITUTE

OF THE

State of Pennsylvania,

AND

MECHANICS' REGISTER.

DEVOTED TO

Mechanical and Physical Science,

CIVIL ENGINEERING, THE ARTS AND MANUFACTURES,

AND THE RECORDING OF

AMERICAN AND OTHER PATENTED INVENTIONS.

NOVEMBER, 1837.

Practical and Theoretical Mechanics and Chemistry.

On Hydraulic and Common Mortars. By General Treussart, *Inspecteur du Génie. Translated from the French by J. G. Totten, Lt. Col. of Eng. and Brevet Col. United States Army.*

Art. III. *Experiments on several hydraulic limes of the environs of Strasburg, on the Metz lime, and on Boulogne pebbles.*

When I was sent to Strasburg in 1816, they were in the habit, there, of using fat lime only. One of the two dikes enclosing the navigable canal which passes from the town to the Rhine across the ditch of the place, requiring repairs, I had occasion to remark that while the two facings, which were of cut stone, were much disjointed, the interior of the dike, which was a mass of concrete, was in good condition, and not a drop of water passed through it. The concrete being very hard, and, as it seemed, not made of brick, or tile dust, I conjectured that it had been composed of hydraulic lime, and I made researches accordingly. I learned that the Millers along the Bruche had used, for a long time, in the repairs of their works, a particular species of lime, which they obtained from a village, at the foot of the Vosges, called Obernai.* Mr. Mossère, Engineer of roads and bridges, told me that he had used this lime in the works of the canal of "*Monsieur*," and that he was well satisfied with it; and, on making the essay, I found it to be eminently hydraulic: it appeared to me to be in no respect inferior to the Metz lime, which I had seen employed with success, at that place, in 1800 and 1801. At other stations, I had several times made hydraulic mortars of fat lime and brick, or tile, dust. At the great works of Vésel, where I was employed three years, considerable use was made of trass, which was brought from Andernach by the Rhine; and on the experience I had acquired of hydraulic limes, I introduced the use of the Obernai lime in all the constructions of the works of Strasburg, both in and out of water. I have already observed that all the revetments comprised between *porte de Pierre* and *pont Royal*, giving a development of about 1650 yards, were made with hydraulic lime. Later researches have shown me that hydraulic lime is to be found at the foot of the Vosges, from Belfort to Vissembourg. I shall give experiments which I made with hydraulic lime from Altkirch, Obernai, Rouxviller, Ingviller, Oberbronn, Verdt, &c.

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* This village is situated between Schelestadt and Strasburg: upon the map of Cassini it is written Ober-Ehnheim, but it is pronounced Obernai, and is so written on some maps.

There are no certain means of knowing by inspection whether a lime-stone will give fat lime or hydraulic lime, for there are hydraulic lime-stones of several colours: those of Alsace are generally of a slate blue, like those of Metz, but those of Altkirch, and the hydraulic chalk of Vitry are white; the Boulogne pebbles, and the English lime-stone of which Parker's cement is made, are red. I may say, however, that when a lime-stone is blue, it is a presumption that it will afford hydraulic lime. It is remarkable that in lime-stone giving fat lime, the iron exists as red oxide, while it exists in the lowest state of oxidation in a great proportion of the hydraulic lime-stones, even when left for a long time exposed to the air; the same thing happens with good slate, and it seems to me to be owing to the presence of clay in the hydraulic lime-stones and in the slate, with which the iron is probably in combination. As I have before said, the Boulogne pebbles, and Parker's cement stone, were exceptions; but this might be due to some peculiar cause: the sea-water might have some influence on the oxidation of the iron contained in the Boulogne pebbles.

All the blue hydraulic lime-stones which I have caused to be calcined, gave a yellow ochre colour whenever the heat was not great. If the calcination was raised, the colour passed, successively, to fawn, ash-gray, and, finally, if the heat had been very great, to slate blue. I cannot satisfy myself as to the cause of the blue colour which the lime-stone takes by a high calcination, because it seems necessary to suppose that this effect is the bringing back of the iron to its first degree of oxidation, which is possible, but does not seem to me easy to explain.

A like effect appears to take place in the calcination of clays that contain oxide of iron. Some clays submitted to calcination are blackish, because of the iron being present at the lowest state of oxidation: at a certain degree of heat, the iron passes to the state of red oxide, and the clay takes the decided colour commonly seen in bricks: if the heat be augmented, the red colour weakens, and becomes fawn or straw colour; if the heat be pushed to a high degree, the clay becomes of an ash colour and afterwards of a very decided slate-blue colour—the iron having then lost a great part of its oxygen. If slate, and the clay just described, be calcined at the same time, the iron will pass with much more difficulty to the state of red oxide in the slate than in the clay. The degree of heat necessary to bring the iron in the slate to the condition of red oxide, will have caused the iron in the clay to pass through all the stages of oxidation before mentioned, and have imparted the blue colour to the clay. Are these different phenomena due to this, that while, in the slate, the iron is combined with the alumine, there is no such combination in this clay? In clays which are strongly calcined, has the alumine the property of abstracting the oxygen from the iron? It is for chemistry to clear up these points.

M. Berthier has given the following method of detecting hydraulic limestones. "The stone must be pulverized and sifted through a silk sieve: 10 grammes of the powder being put in a capsule, muriatic acid, diluted with a little water, must be poured on, little by little. (In want of muriatic acid, nitric acid, or even vinegar, may be used.) The whole should be constantly stirred with a glass or wooden rod, as long as there may be any effervescence; this having ceased, the solution must be evaporated, in a gentle heat, to the consistence of paste; about one pint of water must be added and the whole filtered; the clay will remain upon the filter; it must be dried in the sun or before the fire, and weighed; or, still better, it must be raised to a red heat in an earthen, or in a metallic crucible before being weighed; limpid lime-water must then be thrown into the solution, as long as it forms a precipitate therein; this precipitate should be separated, as soon as possible, by the filter; it is magnesia (sometimes mixed with iron and manganese;) it should be washed with pure water, and dried at a high heat before taking its weight." The weight of the clay retained by the filter, compared with the weight of the lime-stone, will indicate whether it be hydraulic limestone or not. But it might happen that the substance remaining on the filter would be all sand; this may be determined before drying; for if it be sand only, it will be rough to the touch, while if it form a soft and ductile paste, it will be clay, that is to say, a mixture of alumine and silex. Besides, by drying at a high heat, it will be seen if a compact mass be obtained, which will be the case with clay highly heated, or if it be pulverulent matter, which will show that little or no alumine is present. As to the rest, we do not yet know, accurately, in what proportions the silex and alumine ought to be present to constitute the best hydraulic cement. The best manner of ascertaining whether a lime is hydraulic, is the following: take quick lime as it comes, properly calcined, from the kiln; reduce it, with water, to a thick paste, and place enough of it on the bottom of a tumbler to fill it for one-third or one-half of its height; three or four hours after, fill the tumbler with water, and leave all at rest; after two or three days, touch the lime lightly with the finger, to ascertain whether it begins to harden: if it be very hydraulic, it will have taken, after eight or ten days, such a consistence, that no impression can be made on the lime by pressing strongly with the finger. We should assure ourselves whether there has been, in fact, no impression, by throwing off the water and washing the surface of the lime, which will be covered with a thin layer of lime softened by the immediate contact of the water. If the above result be obtained only at the end of twenty, thirty, or forty days, the lime should be regarded as only feebly hydraulic, and if there be no consistence in the lime after the lapse of about forty days, it cannot be regarded as at all hydraulic. This process, which is very simple, is that which I always follow—it was indicated by Mr. Vicat.

Before giving my first experiments, I will explain the processes I followed, both in making mortars, and in breaking them in order to determine their tenacity. In my first experiments, I fixed the proportions of my mortars, by slaking the lime to dry powder with one-fifth of its volume of water, and measuring this lime in powder. Afterwards I measured the lime in paste, in order to approach the mode ordinarily pursued in practice with fat lime. I shall take care to state in every instance, which of these modes of measurement I followed. When I had united the lime to its proper proportion of sand, or other substance, I mixed them well together, adding water, till the consistence was like honey; and I passed the mortar seven or eight times under the trowel. The mortar being made, I put it in wooden boxes which were six inches long by three inches wide, and three inches deep, leaving them in the air for twelve hours, so that the mortars might be somewhat stiffened. They were then placed in a cellar, within a large tub filled with water. I examined the mortars from time to time, and noted the number of days required to harden. I called the mortars hard, when, on pressing them strongly with the thumb, no impression was made on the surface. All the mortars were left in the water one year; at the end of which time they were withdrawn, and I scraped off the four sides or faces with the chisel of a stone cutter until nearly half an inch was removed, when they were rubbed upon a stone until they were reduced to parallelopepids of 6 inches long by 2 inches square. By means of a wooden form which they were made to fit, all were reduced, very exactly, to the same dimensions, and had the four faces well squared. It will be observed that I took off from each side about half an inch, with the view of submitting to rupture only the portion which had not been in contact with water. I ought to notice that in doing this it was often found that the mortars were harder at the surface than in the interior: sometimes the contrary happened. By taking off a portion ($\frac{1}{2}$ inch) from each face, I rejected all, that, from any cause, had received a different degree of hardness from the interior.

To ascertain the tenacity of mortars, the following process was followed. I adapted two pendant, iron, stirrups to a horizontal beam (see plate I. fig. 1.) They were placed parallel to each other and distant four inches in the clear—the lower parts being exactly level. On these stirrups, the parallelopepid of mortar which was to be tested, was placed, passing it through a rectangular collar of iron, a little larger than the prism of mortar, and terminated below by a hook. This collar exercised its pressure upon the mortar by its upper horizontal bar, of which the transverse section had the form of a rounded wedge; and being brought against a bracket, or check, it was midway between the stirrups. To the hook of the collar was suspended a common scale pan, that was loaded by adding successive weights until the prism of mortar broke—which it did with noise. The weights were then summed up, adding, always, 22 lbs. for the weight of the scale pan, cords and collar. In the beginning, as a weight, I poured sand into a box placed on the scale pan, but I renounced this method, because it was too slow. I perceived that it was important to break the mortar promptly; it having several times happened that, when the mortar had supported for some time a weight approaching that which should break it, it would give way, after having taken off a weight of 11 lbs. in order to substitute a greater. On cutting the parallelopepids down to the proper size, I judged nearly of the weight they would sustain; and a weight approaching this was put on at once; after which small weights were added, one after the other, until the mortar broke. When the mortars were capable of sustaining a great weight, I added weights by 10 pounds at a time, because this is a small quantity compared with the total weight, and it is important that the fracture be made promptly. The above are the means I used, and they seem to me to be preferable to any that I have seen described by authors. Some have directed that the mortars should be loaded with weights, until crushed; but the moment when they begin to yield is difficult to judge of, as the angles often break before the middle, and it is not clearly seen when the substance under trial has really yielded to the load. Others have directed that the body of which the strength is to be ascertained, be placed at the end of a strong table to which it is to be fastened; a scale pan is then to be suspended from a portion projecting a certain distance from the table, and weights are to be added till rupture occurs; but it is to be feared that the mortars, or the stones, submitted to this trial, will sometimes project a little more, and sometimes a little less; the weights acting thus at the extremity of unequal arms of levers, will give different results for substances that should exhibit the same tenacity: it is also to be feard that fastening the mortars more or less stifly will influence their resistance.

Mr. Vicat, to judge of the resistance of mortars, employed the following method—reported in pp. 34 and 35 of his memoir: he lets fall upon the surface of the mortar, from a height of 2 inches, a rod of steel slightly conical, and terminated at its end by a plane surface 0.066 inches in diameter; this rod, loaded with the weight of 2 $\frac{2}{10}$ lbs., penetrates a certain distance into the mortar, giving, thereby, the relative hardness.

As it is the resistance that mortars oppose to steady or unimpulsive forces that it is important to know, he thought he might conclude, from some experiments that he reports, that "*the squares of the numbers which express the penetration of the rod are reciprocally proportional to the resistances to the force which tends to break the mortars.*" On this principle he has transformed, by calculation, the penetration of the rod, into numbers proportional to the resistances. I must make, however, on this subject, the following observations.

1st. It is difficult to appreciate exactly the penetration of the rod: 2d. It does not appear to me to be proved, by the few experiments given by Mr. Vicat, that the squares of the numbers expressive of the penetration are always reciprocally proportional to the resistances; 3d. If the rod fall on a grain of gravel, or on a large grain of sand, or even on a grain of lime, we should be liable to infer, from the penetration, conclusions altogether inaccurate as to the resistances of the mortars; 4th. The rod is permitted to fall on the surface of the mortar, and it often happens, as before observed, that the hardness of the surface differs much from that of the interior.

All these causes of error united have conducted Mr. Vicat to conclusions which are sometimes entirely opposite to results obtained in my experiments; I shall take pains to point these out as they present themselves. It will be noticed that to determine the resistance of mortars, I have no calculations to make; by the means I employed, these determinations were perfectly independent thereof, and I have only to enter in the table the number of pounds the mortars are loaded with at the moment of rupture.*

I am now about to present the first results of the experiments made with Obernai lime, and with other hydraulic limes of the environs of Strasburg. The Obernai lime being that which was most often used at the works, I sent a specimen to Mr. Berthier, who analysed it, with the following results: Lime, 0.422; Magnesia and iron, 0.050; Silex, 0.105; Alumine, 0.043; Carbonic acid and water, 0.380. This lime-stone differs but little from that of Metz, which contains as follows: Lime, 0.445; Magnesia and Iron, 0.067; Silex, 0.053; Alumine, 0.013; Carbonic acid and Water, 0.412; loss 0.010.

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* Since the first part of this memoir was written I have seen a new memoir of Mr. Vicat's in which he says, page 117, " We thought ourselves entitled to conclude from a certain number of experiments described in our first work, that the squares of the numbers expressing the penetration of the rod, falling with a given weight, are reciprocally proportional to the relative or absolute resistances of the substance; and, on this principle, the penetrations were transformed by calculation into numbers proportional to these resistances. But, in accordance with the very judicious observations of Mr. Vauthier, Engineer, we have decided to return to the numbers which express the penetration."

I will again observe that it is the tenacity of mortars submitted to unimpulsive or steady forces which it is important to know, and that this will not be obtained by numbers which express the penetration of a rod submitted to an impulsive force.—Av.

Table II.

No. of the mortar.	Composition of mortars.			No. of days required to harden in water.	Weights which they supported before breaking.
	Common bricks of Strasburg . .				462 lbs.
	Refractory bricks of Sufflenheim . .				572
1	Yellow Obernai lime alone in paste,* .			8	372
2	do. do. do. slaked to powder and measured in powder†	1	2½	10	224
	Common sand	1½			
3	Lime the same . .	1	3	12	253
	Sand the same	2			
4	Lime the same . .	1	3½	12	176
	Sand the same	2½			
5	Lime the same . .	1	4	14	92
	Sand the same	3			
6	Lime the same . .	1	3	4	429
	Sand the same	1			
	Trass	1			
7	Lime the same . . .	1	3	4	299
	Trass	2			
8	Gray Obernai lime alone, in paste .			8	354
9	do. do. do. slaked to powder and measured in powder	1	2½	10	400
	Common sand	1½			
10	Lime the same . .	1	3	10	422
	Sand the same	2			
11	Lime the same . .	1	3½	12	187
	Sand the same	2½			
12	Lime the same . .	1	4	15	106
	Sand the same	3			
13	Lime the same . .	1	3	4	473
	Sand the same	1			
	Trass	1			
14	Lime the same . . .	1	3	4	328
	Trass	2			
15	Blue Obernai lime alone, in paste .			20	275
16	do. do. do. slaked to powder and measured in powder	1	2½	14	136
	Common sand	1½			
17	Lime the same . .	1	3	15	154
	Sand the same	2			
18	Lime the same . .	1	3½	16	117
	Sand the same	2½			
19	Lime the same . .	1	4	18	79
	Sand the same	3			
20	Lime the same . . .	1	3	5	378
	Sand the same . . .	1			
	Trass	1			
21	Lime the same . . .	1	3	5	339
	Trass	2			

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* The lime of the hydrates No. 1, 8, and 15 is not of the same burning as the lime of the mortars of the Table. Au.

† I must premise that by the expression, *slaked to powder*, often used in the Tables,

Observations on the experiments of Table No. II.

To make the above mortars I took lime as it came fresh from the kiln, and slaked it to dry powder by throwing on one-fifth of its bulk of water; I left it in this state for twelve hours, and then, having measured the powder, I added the quantity of water necessary to reduce it to paste. I afterwards added to the lime the several quantities of sand and of trass indicated in the table, and I, in all cases, mixed the constituents until a homogeneous mortar was obtained, which required that it should pass six or eight times under the trowel. The mortars were made of the consistence of honey; and were put in the small wooden boxes before mentioned; being lightly compressed with the trowel and the hand, they were left in the air during twelve hours: they had then somewhat stiffened, and in this state were placed in a cellar, in a large tub filled with water: I took care to examine them from time to time, and to note the number of days they required to harden to such a degree that on being strongly pressed with the thumb, no impression was made. At the end of a year they were withdrawn from the water and were broken in the manner described, page 304. In order to institute a comparison between the resistance of my mortars and the building materials of the country, I broke, in the same manner, parallelopepids of bricks, having the same dimensions: the average of the common brick of the neighbourhood of Strasburg, gave me 462 lbs., as is expressed in the table; but the refractory bricks of Sufflenheim, are much stronger, and gave 572 lbs.

As I have before remarked, the Obernai lime is yellow when not fully calcined; when a little more burned it is of a dirty yellow; afterwards it passes to ash-gray; and at last, when too much burned, it is of a slate blue, and pieces of vitrified lime are often found. The lime being burned with wood, it is difficult to obtain a uniform calcination: there are always some pieces of lime too much, and others too little burned. Care was taken to reject the pieces of both extremes; but the mortar made at the works always contained, notwithstanding, lime of the three colours mentioned. If the lime-burners would burn their lime for a little longer time, making a less intense fire, they would not consume more wood and would give better results. But it is difficult to change usages.

The object of the experiments of Table No. II. was to know the degree of calcination which is best, and the quantity of sand most proper to mix with the lime. In table No. 1 where I operated with several kinds of lime, I slaked them to dry powder with a bulk of water equal to half the bulk of lime; but the Obernai lime is well reduced to powder with a fifth of its volume of water. With this quantity there is obtained, moreover, nearly the same volume of lime in powder, as when slaked with the half of its volume of water. In fact, 0.005 of yellow Obernai lime reduced to powder and slaked with one-fifth of its volume of water, gave 0.0105 of lime in powder, and, consequently, 1 part would give 2.10, a result differing little from that of table No. 1.

Table No. II. shows that lime-stone which had been calcined so as to yield a lime slightly gray, was that which gave the best result; that which

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must be understood, lime which was slaked as it came from the kiln, with only a small quantity of water, to reduce it to dry powder. As to lime reduced to dry powder by being left to spontaneous slaking in the air, it is called *lime slaked in the air*, or *air-slaked lime.* I sometimes use in the tables the sign + signifying *more*, and the sign — signifying *less.*—*Author.*

was blue was slow to slake; when reduced to paste it formed with sand and with trass, a mortar susceptible of swelling considerably, because of its preserving, even in this state, the property of absorbing water for a long time. All the boxes containing mortar made of blue lime were totally disjointed by the enlargement of the bulk of mortar. I was not, at first, aware of this effect, and having caused a coat of rough-cast, composed of Obernai lime and sand, to be applied to one of the Government buildings, I was surprised, after five or six months, at seeing, on many points of the surface, swellings of the size, nearly, of a half dollar; these portions finally fell off, and it was then seen that they had been caused by particles of blue lime, about the size of a pea, that were beneath. These particles had not had time to slake thoroughly; and, as imperfectly slaked lime has a strong avidity for water, they absorbed water from the air, and increased in bulk. The force must have been great to break the very hard mortar which covered these particles. This rough-cast has now been on six years; it is very hard, and has perfectly resisted all inclemencies of the seasons. The property which hydraulic lime, too much calcined, has, of swelling considerably after being made into mortar, might be availed of, under certain circumstances. If it were required, for example, to fill cracks in old walls where the hand could not be introduced, or to fill spaces washed from under old foundations, mortar made of highly calcined hydraulic lime might be advantageously used, because the mortar by swelling would more perfectly fill the cavities. But especial care should be taken not to use it in new constructions, as it might cause serious accidents. It is stated that two locks having been made, a short time since, with lime that had not been sufficiently slaked, the mortar swelled to such a degree that all the cut stone was displaced, and it was necessary to rebuild the masonry. This accident happened to the works for the improvement of the navigation of the Vésère.

Table No. II. teaches that the yellow lime and the blue lime gave, alone, greater resistances than when mixed with sand. But I ought to observe that no conclusion should be drawn from this, as the three experiments on hydrates were made with lime different from that used in making the mortars of the table. At the time of making these experiments, I was far from thinking there could be so great a difference between mortars made of pieces of lime which seemed to have sustained the same degree of calcination. The best results with the Obernai lime alone, are 372 lbs. for the yellow, 354 for the gray, and 275 lbs. for the blue. I regret not being prepared for this superiority of the hydrate over mortars, but I was far from expecting it: otherwise, after having made the experiments with the limes alone, I should have made mortars, by adding successively, $\frac{1}{4}$, $\frac{1}{2}$, $\frac{3}{4}$, &c. of sand, so as to judge better of the effect of sand; but it was only on breaking the mortars at the end of a year, that I could know the effect, and I have often, on such occasions, obtained results that surprised me, and been conducted to new experiments, requiring another year to furnish results.

It is seen that after a certain quantity of sand has been added, the mortars lose much of their tenacity: and that for every degree of calcination, trass considerably augments the resistance. It is remarkable that mortars made of lime, sand, and trass, are in general better than those made with lime and trass, without sand: I have, however, found some exceptions. Mr. Vicat thinks that puzzalonas only slightly energetic, suit best with hydraulic limes; but my experiments do not support this opinion; as the trass I used was very energetic, and it will be seen that I always had very good results on mixing it with very hydraulic lime. I obtained, in the above table, better results with Obernai lime, trass, and sand, than with Obernai lime and trass without sand, and it will be seen, by experiments which follow, that I found the like results with fat lime. In the 7th number of the *Memorial de l'Officier du génie*, I have advanced that, in important constructions, it is not prudent to employ hydraulic lime without a little trass. Mr. Vicat is of a different opinion: for we see a note in the *Annales des Mines*, Vol. X., page 501, wherein he says that the success which has attended the use of hydraulic lime in different works which he cites, ought to suffice to undeceive those who partake my fears as to the insufficiency of hydraulic lime without trass, in important constructions. I have, nevertheless, had an opportunity of seeing the concretes used at the canal *Saint-Martin*, at Paris, and at the basin of the *Palais Royal*, made of the hydraulic lime manufactured by Mr. Saint-Léger, and they are much less hard than those made at Strasburg. I do not doubt, however, that the concrete employed at the *Saint-Martin* canal will well fulfil its object. Being obliged to cover all the bottom of this canal, which has an extent of about 4400 yards, with concrete, in order to keep the water from filtering into the cellars of the inhabitants, it would have much augmented the expense, to have added to this mortar, cements analogous to trass. Concrete composed of Obernai lime and sand, without trass, was often used at Strasburg, for the foundations of revetments, of piers of bridges, &c.; but by "important constructions" I meant Locks and Dams, which have a great pressure of water to sustain, perhaps for a long time. There is no doubt that if such a work were executed with gray lime like that of the preceding table, there would be nothing to fear: but this would require the rejection of a great part of the kiln, and would make the mortar very dear: the extremes, only, can be rejected, so that the lime used is a mixture of the three qualities. If a mean be taken of the three kinds of mortar found under Nos. 3, 10, and 17, we shall have 276 lbs. for the result, while the mean term of the three Nos. 6, 13, and 20, is 427 lbs. I have often, however, had results much weaker than those of the above table, as will be seen in the sequel. I have even had occasion to remark that hydraulic limes coming from the same quarry, and appearing to be calcined nearly to the same degree, have, nevertheless, given very different results: in these cases, the lime, which, mixed with sand had given me a weak resistance, had always afforded a very good mortar, when I used sand and trass with the lime. I am convinced that these last mortars have given, on an average, a resistance more than double the first; I persist, therefore, in thinking that in important constructions, such as Locks and Dikes, which have constantly a strong pressure of water to support, it is prudent to put a little trass, or some analogous substance, into the mortar, even when the hydraulic lime used is as good as the very good hydraulic lime of Strasburg. It is for the Engineer who has the work to execute, to examine the quality of the lime he is to employ, in order to fix the quantity of these matters to be added in the mortar. In my opinion a Government should not regard a light additional expense, when the object is to obtain constructions that will last a long time without needing repairs: unfortunately the contrary system, which I think a very bad one, most commonly prevails.

I ought to premise that in the experiments which I shall report, I shall abstain from conclusions too general. With the limes of Alsace I have often noticed contradictory results, and even, as before mentioned, with limes from the same quarry. We have not, as yet, collected enough facts to establish a general theory: that which may be true as to the limes of one country, may not be true as to those of another. I give the results which I have obtained. It is very desirable that Engineers who have some leisure, should make experiments; as it is only when a great many results shall have been collected that we shall be able to deduce any general principles: those which I shall present must be considered as belonging only to the limes which I shall use.

I now proceed to report other essays upon other limes of the environs of Strasburg.

Table III.

No. of the mortar.	Composition of the mortars.			No. of days which they took to harden in wat'r	Weight which they supported before breaking.
1	Altkirch lime alone, in paste			10	268 lbs.
2	do. do. slaked to powder and measured in powder	1	3	12	174
	Common sand	2			
3	Lime the same	1	3	4	539
	Sand the same	1			
	Trass	1			
4	Lime the same	1	3	4	535
	Trass	2			
5	1st Villé lime alone, in paste			25	119
6	Lime the same, slaked to powder and measured in powder	1	3	28	68
	Sand	2			
7	Lime the same	1	3	4	473
	Sand	1			
	Trass	1			
8	Lime the same	1	3	4	473
	Trass	2			
9	2d Villé lime alone, in paste			25	110
10	Lime the same, slaked to powder and measured in powder	1	3	28	114
	Sand	2			
11	Lime the same	1	3	4	396
	Sand	1			
	Trass	1			
12	Lime the same	1	3	4	475
	Trass	2			
13	Rosheim lime alone, in paste			6	484
14	Rosheim lime, slaked to powder and measured in powder	1	3	14	209
	Sand	2			
15	Lime the same	1	3	4	462
	Sand	1			
	Trass	1			
16	Hochfeld lime alone, in paste			12	207
17	do. do. slaked to powder and measured in powder	1	3	22	136
	Sand	2			
18	Lime the same	1	3	4	429
	Sand	1			
	Trass	1			

Table III.—(continued.)

No. of the mortars	Composition of the mortars.			No. of days which they took to harden in wat'r	Weight which they supported before breaking.
19	Verdt lime alone, in paste .			8	484
20	do. do. slaked to powder and measured in powder . . Sand . . .	1 2	3	10	279
21	Lime the same . . . Sand . . . Trass	1 1 1	3	4	510
22	Lime the same . . Trass	1 2	3	4	310
23	Oberbron lime alone, in paste .			10	264
24	Same lime measured in paste . Sand . . .	1 2	3	10	220
25	Same lime do. . . Sand . . .	1 2½	3½	8	352
26	Yellow Bouxviller alone, in paste .			10	299
27	Gray do. do. do. .			30	99
28	Blue do. do. do. .			40	79
29	Yellow do. measured in paste Sand . . .	1 2	3	10	286
30	Lime the same . . . Sand . . .	1 2¼	3¼	10	187
31	Lime the same . . . Sand . . .	1 2½	3½	10	165
32	Lime the same . . . Sand . . .	1 2¾	3¾	12	132
33	Lime the same . . . Sand . . .	1 3	4	12	99
34	Gray Bouxviller lime, measured in paste Sand . . .	1 2	3	25	77
35	Blue do. do. do. . Sand . . .	1 2	3	30	66
36	Yellow do. do. do. . Sand . . . . Trass	1 1 1	3	5	407
37	Metz lime alone, in paste .			15	169
38	Metz lime slaked to powder and measured in powder . . Sand . . .	1 1½	2½	16	312
39	Lime the same . . . Sand . . .	1 2	3	16	262
40	Lime the same . . . Sand . . .	1 2½	3½	16	176
41	Lime the same . . . Sand . . .	1 3	4	18	143
42	Lime the same . . . Sand . . . Trass	1 1 1	3	4	466
43	Lime the same . . Trass . . .	1 2	3	4	262
44	Lime the same . . Trass	1 3	4	4	385

Observations on the Experiments of Table No. III.

The Altkirch lime comes from a lime-stone which is grayish: all the others from stones that are blue like those of Obernai and Metz. All were slaked to powder with one-fifth their volume of water, and left in that state for twenty-four hours, before making them into mortars. Several being too remote to be brought into use at the works, I contented myself with some experiments on their hydraulic properties. It is seen that the Altkirch lime is good. The piece of lime brought me had, probably, been out of the kiln a number of days, which should considerably influence the result, as will be soon seen. It is possible, therefore, that this lime is better than the experiments indicate. Two different kinds were brought from Villé. I was told that it was as good as the Obernai lime. The first kind is brown, and the second grayish: we see that the resistances are feeble. The limes of Rosheim, Hochfeld, Verdt, Oberbronn, and Bouxviller gave very good results whether alone or in mortar. It will be seen, also, that with the limes slaked to powder and measured in that state, the mortars consisting of lime and sand, generally supported less weight than the lime alone. It is possible that there was too much sand in them. I began with the principle, admitted by several Engineers, that it is better to err by excess of sand than by excess of lime; but it appears this is not always true, for the experiments of tables Nos. II. and III. show that when the mortars had a little too much sand, their resistance was much diminished. It is possible also that some hydraulic limes may become harder when alone, than when mixed with any proportion of sand whatever.

The Oberbronn lime was measured in paste; it appears that with two and a half parts of sand, it gave a better result than with two parts only, and that this result was superior to that obtained with the lime alone.

As the Bouxviller lime might be used at Strasburg I treated it with more detail. We see that this lime loses much by being too much burned, and that it cannot support as much sand as that of Oberbronn; and, also, that with all these limes, the mortars were much improved by adding trass to the lime and sand. It is an error, therefore, to suppose that energetic puzzalonas are not adapted to good hydraulic limes. Tables Nos. II. and III. show that different limes take very different proportions of sand. Thus, as before observed, various experiments induce me to believe that pieces of lime from the same quarry may give very different results, although calcined to the same degree. It also appears that different degrees of burning require different proportions of sand; it becomes, therefore, difficult to fix accurately the proportion of sand which should enter the mortar.

The above experiments were a first essay only: I proposed making others, with a mixture of lime calcined to different degrees, and to repeat the experiments on several burnings, so as to be able to take a mean. At Strasburg we adopted one part of lime to two and a half parts of sand. The mortar was good, but it, possibly, was not the best that could be made. Supposing the best proportions, as to hardness, were one part of quick lime to two parts of sand, it would still remain to determine whether these proportions should be adopted; because, if in putting a little more sand the result should be but slightly inferior, it might be done unhesitatingly, if the economy were sensible. I was about to make these experiments on the different limes of the environs of Strasburg, when I was obliged to quit the place, in consequence of the new functions to which I was called.

Towards the end of the table, are the experiments made with Metz lime.

It appears that one part of lime in powder, and one and a half of sand, is the mixture which gave the best result, and that it is stronger than the lime alone. Notwithstanding the mortars were made in the month of June, the hardening was slow; and experience teaches that mortars harden sooner in this season, than in winter. The comparisons of No. 42, 43 and 44, show that for the hydraulic limes of Alsace, better results are obtained with sand and trass, than with trass alone. No. 44 which was made with one part of lime in powder, shows that this lime will bear a good deal of trass without losing much of its resistance.

In the above table, the limes used alone, and in the mortars, came from the same piece of limestone, in each case.

I shall conclude my observations on Table No. III, by saying that in the course of my experiments, I have had occasion to observe, more than once, that discordant results were obtained when I used lime that had been slaked for several days. When I made the experiments with the Oberbronn lime, Nos. 23, 24 and 25, I put aside a portion of the lime in powder which I had used. At the end of 15 days, I repeated the experiments recorded at Nos. 23 and 25: for No. 23, which was lime alone, the hardening required 30 days instead of 10 days, which were necessary, when the fresh lime was used. At the end of the year, the piece of hydraulic lime, broke with a weight of 154 lbs, while No. 23, which was made immediately, supported, before breaking, a weight of 264 lbs. As to the mortar made of one part of lime, measured in paste, and two and a half parts of sand, the same as mortar No. 25, the hardening required 25 days instead of 8 days, and the mortar broke with a weight of 154 lbs, while that of No. 25, made immediately, supported 352 lbs, before breaking. I put this lime aside for fifteen days, to see if an old saying of masons, that lime loses its energy in the air, had any foundation. The experiment convinces me that the saying is true, at least as regards the hydraulic limes that I treated; but I wished to see, also, what results I should obtain, with mortars made in the same manner and of the same piece of lime, but at different times. I made, in consequence, the following experiments.

Table No. IV.

No. of the series.	Composition of the mortars.			Made immediately.		After six weeks.		After 2½ months	
				hardening.	w't. support'd	hardening.	w't. support d	hardening.	w't. supportd
	Yellow Obernai lime slaked to powder and			days	lbs	days	lbs	days	bs
1	measured in powder.	1	3	9	187	15	44	25	22
	Sand	2							
	Lime the same . . .	1							
2	Sand	1	3	6	407	6	407	6	297
	Trass . , ...	1							

Observations on the experiments of Table No. IV.

To make the above experiments, I took the yellow Obernai lime, and slaked it to dry powder by throwing on ⅕th of its volume of water. I gave it time to cool, and then made two mortars: one, with one part of lime measured in powder and two parts of sand; the other with one part of lime in powder, one part of sand, and one of trass. The mortars remained in the air during twelve hours, and I then put them in water, where they remained one year, at the end of which time they were broken. The table shows that the mortar of lime and sand, hardened in 9 days and supported 187 lbs, before breaking; it shows also, that the mortar containing lime, sand and trass, hardened in 6 days, and supported 407 lbs. At the end of a month and a half, I repeated the same experiment, using, in powder, the same lime which had served in my first experiment, and which had been lying in an open vessel. I followed, for the second experiment, the same process as for the first: the table shows that in the mortar composed of lime and sand, the hardening was slower, and the resistance much less; and that for the mortar containing trass, the result was the same: again, at the end of two months and a half, I repeated the experiments, using the same lime slaked to powder. The table shows that the hardening was still slower, and that the resistance was still less in the mortar, composed of lime and sand; for by the expression -22, is meant that it could not sustain the weight of the scale-pan, cords, and collar, which was 22 pounds. It also shows that the mortar containing trass experienced a sensible diminution in its resistance, although the time of hardening was the same; but the diminution was not as great as in the mortar made of lime and sand only.

I have thus far gone into particulars, in order that there might be no doubt as to the manner of proceeding: in the following tables, I shall give fewer details. I repeated the above trial, as shown in the following table, using another piece of lime.

Table No. V.

No. of series	Composition of the mortars.	Made immediat'ly		After 15 days.		After 25 days.		After 35 days.		After 45 days.		After 2 months.	
		H	W	H	W	H	W	H	W	H	W	H	W
		d's	lbs 1	d's	lbs	d's	lbs	d's	lbs	d's	lbs	d's	lbs
	Obernai lime slaked to powder and measured in powder. . . 1 Sand . . 2 } 3	25	264	11	121	15	77	18	88	18	77	20	88
2	Lime the same . 1 Sand . . . 1 Trass . . . 1 } 3	12	473	5	330	6	297	10	286	12	304	12	299

I made the above experiments, in the same manner as has been explained of Table No. IV; but it will be seen that the periods at which the mortars were made, were brought nearer together. Each experiment in the above table, is divided into two columns: the first contains the number of days, the mortar required to harden, and is marked H; the second column, marked W, expresses the number of pounds that the mortar supported before breaking. I shall adopt, hereafter, this order for all the tables which follow, whenever they contain many columns.

The piece of lime which I used for these experiments, was a petrified cornu ammonis, which was 20 inches in diameter. The table shows that with the first mortar, of lime and sand, the hardening was quite slow, which might be owing to the mortar being made so late as the month of October. It is remarkable that the hardening was not so tardy, for the following, which were made 15 days after slaking. As to the resistance, it was the greatest with the mortar made immediately. That made after fifteen days, lost about half its strength: beyond this period the mortars offered a resistance varying from 77 to 88 lbs, and the hardening was slow.

If the mortars containing lime, sand and trass be reviewed, it will be remarked that the resistance was greatest, with that made immediately, and that it, afterwards, went on diminishing, but keeping well elevated; and that the hardening of the mortar made at the end of fifteen days, was the most prompt.

When making the experiments of the preceding table, I put aside a piece of the same lime, to allow it to slake spontaneously in the air: after two months I made mortar with it, and obtained the following results: it did not harden till the end of twenty-five days, and the mortar broke with 55 pounds, while the table shows that mortar made with the same lime slaked to powder, and which had been left in that state, during the same time, supported a weight of 88 lbs: this result induced me to recommence the experiment with another piece of lime. I proceeded in this way: I divided the lime into two parts; slaking one of these parts into dry powder, by throwing on a fifth of its volume of water, and leaving the other part to slake in the air. I immediately made two mortars with the lime that had been slaked to powder; in one, I mixed sand only with the lime, and in the other I put sand and trass, and at different periods, I repeated the same experiment. It was not till after fifteen days, that the piece of lime which I had left in the air, gave enough lime in powder to begin two similar sets of mortars. The table, following, shows the results I obtained.

Table No. VI.

No. of series	Composition of the mortars.	Made immediat'ly		After 15 days.		After 1 month.		After 2 months.		After 3 months.		After 4 months.	
		H	W	H	W	H	W	H	W	H	W	H	W
		d's	lbs	d's	lbs	d's	lbs	d's	lbs	d's	lbs	d's	lbs
1	Obernai lime slaked to powder and measured in powder 1 Sand . 2 } 3	10	121	6	132	4	132	5	117	5	70	6	66
2	Same lime air-slaked and measured in powder . 1 Sand . . 2 } 3			+40	77	+40	44	+40	33	+40	-22	+40	-22
3	Same lime slaked to powder and measured in powder 1 Sand . . 1 Trass . . 1 } 3	2	209	2	(1)	2	389	2	352	2	317	3	195
4	Same lime air-slaked and measured in powder . . 1 Sand . . 1 Trass . . 1 } 3					6	352	5	319	6	275	6	455

(1) This mortar broke on being cut; it was very hard, but I could not determine its resistance. Au.

Observations on the Experiments of Table No. VI.

This table shows, first, that the piece of lime used was not of the best quality. I, however, selected, to make the experiments of Table Nos. IV, V and VI pieces of lime of a yellow fawn colour, which appeared to have been submitted to the same degree of calcination. The trials of tables Nos. IV and V, were commenced in the month of October, while those of No. VI, were begun in June; the febleness of the first result of table No. VI cannot, therefore, be attributed to a cold season of the year; nor can it be attributed to weather too warm, because the experiments of table No. II, which gave very good results, were made in June. We are obliged, therefore, to infer, as I have already noticed, that pieces of quick lime of the same quarry, are far from having, all of them, the same quality.

The first mortar of lime and sand, augmented its resistance a little, after fifteen days slaking, and its hardening was much more prompt after a month. Beyond this term, the time of hardening went on augmenting, and the resistance diminishing, in a sensible manner, as in Table No. V. It will be seen that for the mortar made, in the same manner, with air slaked lime, the hardening was very slow: I pursued the examination for 40 days, and the mortars were not yet hard. The table shows that after 15 days, the mortar gave a resistance much less than that of which the lime had been slaked to powder, and that the resistance went on diminishing. At last it was so weak, that, after three or four months, the mortars were unable to sustain the scale-pan, &c., weighing 22 lbs: the mortars had, in the mean time acquired some consistence, but it was so feeble as to be easily broken between the fingers.

The table shows, also, that mortar composed of lime slaked by water to dry powder, united with sand and trass, hardened promptly. The resistance was less in the mortar made immediately; but it afterwards augmented, and then diminished, and finally, the last gave a great increase. I do not know to what to attribute the anomalies observable in the resistance of these mortars. The last series, made with lime slaked in the air, and in which there is one part of Trass, gave a good result, and the hardening was quite prompt; but the resistance was always less than that of the mortar of which the lime had been slaked by water, to powder. This last mortar also presented, after four months, an anomaly which I cannot explain.

It will be seen by the results of the three tables, Nos. IV, V, and VI, that the presence of trass, corrected the evils resulting from the exposure of the lime to the air, after having been slaked by either process. The air-slaked lime had lost almost all its hydraulic properties; but on mixing it with trass it gave, notwitstanding, very good mortar: this is not surprising, since it appears that exposure to the air, has no other effect, than to cause hydraulic lime to pass to the state of common lime, and this last gives very good mortar when mingled with sand and trass.

The importance of these results, induced me to engage in similar experiments with the Metz lime: they are found as follows.

Table No. VII.

No. of series	Composition of the mortars.	Made immediat'ly.		After 1 month.		After 2 months.		After 4 months.	
		H	W	H	W	H	W	H	W
		d's	lbs	d's	lbs	d's	lbs	d's	lbs
1	Metz lime slaked to powder and measured in powder . . . . 1 Sand . . . . . . 2 } 3	20	116	30	44	30	22		–22
2	Same lime slaked in air and measured in powder . . : . 1 Sand . . . . . . 2 } 3			35	88	15	79	9	88
	Same lime slaked to powder and measured in powder . . . 1 Sand . . . . . . 1 Trass . . . . . . 1 } 3	3	180*	5	312	11	319	11	385
4	Same lime air-slaked and measured in powder . . . . 1 Sand . . . . . . 1 Trass . . . . . . 1 } 3			13	352	9	209*	7	440

Observations on the experiments of Table No. VII.

I made the above experiments like those in the preceding table. If the first mortar, alone, be compared with No. 39 of Table No. III, it will be seen that the lime used in making the above mortars, was much inferior to that of table No III, although the limes appeared to have been burned to the same degree. The trials, in both these tables, were made in the middle of summer, the quantities of sand and all the other circumstances, were the same; nevertheless, the first mortar of table No. VII, shows a resistance of only 116 lbs, while the similar mortar, No. 39 of table No. III, gives 262 lbs.

It is seen that the first series, composed of lime and sand, shows the greatest resistance when the mortar was made immediately, and that the resistance went on diminishing. At the end of four months, this mortar could not support the scale-pan, and it broke easily in the fingers. We see, therefore, that with Metz lime slaked to powder, the result is the same as with Obernai and Oberbronn lime.

The second series was begun, at the end of a month, with the same lime left to slake in the air: the result varied from 79 to 88 lbs.—a resistance inferior to that obtained by making the mortar immediately: but on comparing these results with those of table No. VI, we see that the Metz lime, when air slaked, gave better mortars, than when slaked to powder with water; while it was the contrary with the Obernai lime, of which the two first series of table No. VI were made. It is also remarkable that the hardening was more and more prompt, with the air-slaked Metz lime, while it required more than 40 days for the Obernai lime to harden, in each case.

As to the two series, of table No VII, made with lime, sand and trass, good results were obtained, whether the lime was slaked with water to powder, or was air-slaked; in both cases, the resistances went on increas, ing, and the mortars made of air-slaked lime, gave better results than those of lime slaked to powder, which was the contrary of the results of table No. VI.

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* These mortars were split in the middle.

It is to be regretted, that in the first experiment, of the above table, made with lime, sand and trass, the prism of mortar was found split, and separated into two parts. I put the two pieces together, before breaking; but the resistance must have been diminished by the circumstance, and it amounted only to 180 lbs. The last series, also, gives a mortar sustaining 209 lbs. forming an anomaly, when compared with that which precedes, and with that which follows: this mortar was cracked, but the parts were not separated.

It will appear from all the experiments reported, in tables Nos. IV, V, VI and VII, that the saying of masons, that lime loses its energy in the air, is not, as regards hydraulic lime, without foundation. All those which I have treated above have passed to the state of common lime, after some months, when they had been slaked with water; and the Obernai lime gave the same result when air-slaked. I have sought for the cause that might transform hydraulic lime into common lime.

Chemistry teaches us that lime has not the property of absorbing a new quantity of oxygen; but it is not with lime, properly speaking, that we make mortar; it is with the hydrate of lime; that is to say, with lime slaked by water, which enters into combination with it, forming a new body, which may have new properties. It, therefore, occurred to me that hydrate of lime, or, in other words, slaked lime, might absorb oxygen.

In verifying my conjectures in this respect, I could not employ hydraulic lime, because all those I had at command, contained metallic substances capable of oxidation. I proceeded, therefore, in the following manner: I took a piece of white marble which I assured myself contained almost no iron; I had it calcined in a kiln; as soon as it came from the kiln, I pulverized it, and divided the powder into two equal parts, one of these parts was slaked with a little water, and the other was left in the state of quick lime. I put the slaked lime, and the quick lime, each into one of those cylindrical glass vessels used to measure liquids: I placed each of these vessels in a plate containing a little water. I then covered the two vessels containing the lime, with two other vessels, of the same kind, a little larger—the mouths of these covering vessels being immersed in the water. By these means the lime was no longer in connexion with the atmosphere, and it was connected with the water, only through the column of air that was between the two vessels. After five or six days, the water had mounted perceptibly, between the two glass vessels, which contained the slaked lime, and at the end of twenty days, it had mounted four inches. It must be noticed that at this time, the water had risen very little between the two glasses, containing the quick lime. I repeated this experiment several times, and always with the same result. It, therefore, appeared to me that there had been a great absorption of oxygen by the slaked lime, and a slight absorption by the quick lime. I communicated the results I had obtained to Mr. Coze, professor of Pharmacy at the academy of Strasburg, and requested him to ascertain the quantity of oxygen that had been absorbed in the two cases. To that end, a cylindrical glass, graduated, vessel was taken, and one measure of atmospheric air and two measures of deutoxide of azote (nitrous gas) were introduced. As soon as the air in the cylinder had become red, and the water had risen therein, the residue stood at 112 on the scale of the cylinder. The same thing was done with the air that had been in contact with the quick lime: this gave also a red colour, and the residue stood at 117 of the scale. And then the same trial was made with the air that had been in contact with the slaked lime: the red colour was very feeble, and the residue stood at 132 of the scale. In this last case, therefore, there had been a great quantity of gas absorbed by the lime which had been slaked with a little water. To be convinced that it was oxygen, that had been absorbed, hydrogen gas was mixed with the remaining gas, and the mixture submitted to the electrical spark, without any detonation; from which it resulted, that almost all the oxygen had been absorbed by the hydrate of lime.* That the quick lime absorbed but little oxygen, was doubtless owing to this, that this lime could form a hydrate only very slowly, by absorbing water from the dish through the column of air. It appears to me, then, that it is to the absorption of oxygen we should attribute the singular result, that is obtained by leaving exposed to the air, hydraulic limes that have been slaked with a little water. It has been seen that all the above limes, thus treated, have lost, after a little time, almost all their hydraulic properties, and have passed to the condition of common lime.

It appears to me difficult to explain, why the Metz lime which was slaked in the air gave, at the end of four months, a better result than the lime which had been slaked to powder, while the opposite results were obtained with the Obernai lime. Can this difference be owing to the Metz lime containing the oxide of manganese, which the Obernai lime does not? We know that when oxide of manganese is heated, a great part of its oxygen is disengaged, and that it afterwards absorbs it from the air. When the limestone containing it has been calcined, it is possible that this oxide prevents the lime from absorbing oxygen by its own greater affinity for it. It will be interesting to ascertain if the same thing happens with all hydraulic limes, containing oxide of manganese.

It will be seen by the above experiments how important it is not to leave exposed to the air for any time, hydraulic limes which have been slaked to powder, unless well assured that they are not of the same nature as those which become, in this way, common limes. The same observation applies to hydraulic lime slaked spontaneously in the air: we ought to be certain that the same effect will not result as in the Obernai lime of Table No. VI. Without this precaution we shall be liable to make very bad mortars out of very good materials; and to cause very important and expensive constructions to fail. If good results were obtained in tables Nos. VI. and VII. with trass mortars, of which the lime had been slaked for a long time—whether with water to powder or spontaneously in the air, it was because common lime always gives good mortars with trass, whatever may have been the slaking process, as will be seen in the sequel. The above experiments on the absorption of oxygen by hydrate of lime, are contained in a memoir on hydraulic mortars which I addressed to the Committee on Fortifications in October, 1823; an extract from that memoir was printed in 1824, and inserted in the 7th number of the Memorial de l'Officier du génie.

It remains, before closing this article, to speak of the pebbles of Boulogne, of which, having heard, I caused specimens to be brought to Strasburg: but before reporting the experiments therewith, I must refer to a species of lime, much resembling them, well known in England.

In 1796 Messrs. Parker and Wyatts obtained a patent in London for making a particular kind of lime, which was first called *aquatic cement*, and afterwards, improperly, *Roman cement.* At this moment, this cement is an article of considerable commerce, and is sent even to the Indies. This substance has the property of solidifying, like plaster of Paris, almost instantly, when used in the air, and of taking, very soon, a strong consistence in water. It is mixed with 2, 3, 4, and 5 parts of sand, to 3 parts of cement, according to the use to be made of it.

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*It appears from this, that there is reason for forbidding the occupying of newly plastered houses—as they must be unhealthy. Au.

Captain Le Sage, of the Engineers, has brought to notice a substance like the English stone: it was found by an Englishman, by accident, about 20 years ago, upon the shore of Boulogne: a description is given of it in the 2nd vol. of the *mémorial de l'Officier du génie.* This substance is thrown upon the shore by the sea, in the form of pebbles, seldom weighing more than 2½ lbs. Their colour is, in general, a yellowish and dirty gray: several of these pebbles have the surface, to the depth of $\frac{8}{10}$ of an inch, of a rust colour: these stones are of a very fine grain, and they are hard. They are found only in small quantities, and here and there, so that they are used at Boulogne only, where they are advantageously employed in hydraulic works. Mr. Drapier made the analysis of these, and Mr. Berthier of the English stone: from a comparison of these two stones, presented by Mr. Berthier in the same table, after deducting the carbonic acid, and the water which is evaporated by calcination, there remains of the substances employed as lime, viz:

The English stone—lime, 0.554;	clay, 0.360;	oxide of iron, 0.086.
The Boulogne pebbles— do. 0.540;	do. 0.310;	do. 0.150.

I ought to add that the analysis gave for the English stone some thousandths of carbonate of magnesia, and of manganese, which are not found in the Boulogne stones: but these quantities being very small, we see that the two are essentially the same. I now give a table of the experiments made with the Boulogne pebbles.

Table VIII.

No. of the mortar.	Composition of the mortars.			No. of days required to harden in water.	Weight supported before breaking.
				days.	pounds.
1	Boulogne pebbles alone, ordinary burning,			½	108
2	do.			1½	132
3	do.			1½	158
4	do. Sand	2 1	3	20	37
5	Pebbles as above Sand	2 1	3	22	33
6	Pebbles as above Sand	1 1	2		22
7	Pebbles as above Sand	1 2	3		— 22
8	Pebbles as above Sand Trass	1 1 1	3		22
9	Pebbles as above Trass	1 2	3		26
10	Pebbles alone, air-slaked for a month			15	48
11	Pebbles alone, slaked a month with one-fifth the volume of water.				— 22
12	Pebbles alone, highly calcined			12	286

Observations on the Experiments of Table No. VIII.

The lime of the first eleven numbers of Table No. VIII. was calcined in a lime kiln, being placed with lime-stone producing fat lime: it was, therefore, calcined with the degree of heat required for burning fat lime. The first three numbers are of the lime alone. We were obliged to pulverize the pebbles as they came from the kiln, because, on throwing on water, instead of being reduced to powder, like fat limes, and ordinary hydraulic limes, they absorb and solidify it. These pebbles afford no vapours on slaking, and give out very little heat. This lime requires less water, than ordinary hydraulic lime, to be reduced to paste. In the above experiments, the pebble lime was reduced to paste a little stiffer than for ordinary hydraulic mortar, because it was perceived that this lime required less water. The first three numbers were made in the same manner; differing only in this, that the first number was put in water after having been formed into paste and exposed three hours in the air; the second number, after two hours exposure, and the third, immediately: it will be seen that this last gave the greatest resistance, but that which gave the least resistance was the quickest to harden, which arose from its having already acquired in the air an evident consistence. We see then, that in constructions in water, when this lime is to be used, it is important to prepare no more mortar than can be used immediately. The numbers from 4 to 9, inclusive, are mixtures of pebble lime with sand in several proportions, and the numbers following contain trass. The hardening was so slow that I was unable to observe them all. We see that the resistance of all these mortars was feeble. It is singular that trass, which gave such good results with all the hydraulic limes I have tried, gave such feeble results with this pebble-lime: it appears that this lime does not bear, with advantage, any mixture. The pebbles of No. 10 were broken up and left to slake in the air for a month; those of No. 11 were exposed during the same time to the air, after having $\frac{1}{5}$th of their bulk of water thrown on them. We see that the air-slaked lime gave a very weak result, and that which was slaked with water gave a still feebler, since it was unable to sustain the weight (22 lbs.) of the scale pan, &c.

The weak resistances, shown in the first numbers of the above table, gave me the idea of another experiment, after a higher calcination: to this end, I exposed a piece, in a lime kiln, opposite one of the flues intended to convey the heat through the kiln: the pebble was highly burned; it took a deep rust colour, while those less calcined were of a yellow fawn. No. 12, which is the experiment made with the highly burned pebble, was treated like No. 2, being left, in the state of paste, in the air for one hour before being immersed. We see that the resistance was very good, since this No. 12 supported the weight of 286 lbs. before breaking; but the hardening was quite slow. This trial was made with the remainder of my stock of pebbles; otherwise I should have made several essays to ascertain whether by mixing this strongly calcined lime, with sand and trass, I should obtain better results than those given when less burned.

Taken all together, the experiments of table No. VIII. show, that the lime derived from Boulogne pebbles is a hydraulic lime presenting in several respects, great differences from the other hydraulic limes which I have treated. The ordinary hydraulic limes of which I have spoken, demand only the same degree of heat as is required to calcine fat lime, and it is perhaps proper to calcine them a little less. When these limes are but little burned, they form a good body with sand, and give very good results with trass: if the same degree of heat be given as was received by the pebbles in experiment No. 12, they sustain an incipient vitrification, and give only weak results. A circumstance, quite remarkable, is, that when I treated these limes without mixing them with any other substance, those numbers which hardened the quickest, gave the least resistances, while those which took the longest time to harden, gave results much superior to the others. All these experiments were, notwithstanding, made at the same season—the beginning of the winter. The differences which I have pointed out, between Boulogne pebbles and ordinary hydraulic lime, induce me to suspect there might be, in the pebbles, some substance that escaped detection in the analysis. I have, moreover, attempted to recompose this lime, by a mixture of the substances given by the analysis: on burning it, I had ordinary hydraulic lime, and none of the peculiar results given by the pebbles. To ascertain if the pebbles contained any substance not mentioned in the analysis, I took one of the crude stones, broke it, and examining it with the magnifier, I noticed some very small crystals which did not appear to be calcareous. I pulverized these particles and put them, for some time, in distilled water: I then filtered and concentrated the liquid: it had a decided salt taste, and on leaving it at rest, I obtained quite a quantity of crystals of muriate of soda, which had been previously noticed as being present. It appeared to me that this muriate of soda, contained some crystals of carbonate of soda. This did not surprise me, since we know, from the fine observations of Mr. Bertholet, that Natron, which is brought from Egypt, is formed by the decomposition of muriate of soda, by the carbonate of lime which exists in the bottom of the lakes, situated in the desert of Thaïat to the west of the Delta. In winter, these lakes fill with salt water which enters through the bottom and rises about seven feet; on the return of warm weather, the water of the lakes is completely evaporated, leaving on the bottom, which is calcareous, a stratum of natron, which is a mixture of sea salt, (muriate of soda) sulphate, and carbonate, of soda, constituting the soda of commerce: this natron is broken up from the bottom with iron bars. The muriate of soda which exists in the Boulogne pebbles in notable quantity, and the crystals of carbonate of soda which I thought I discovered, induced me to make several experiments (which I shall soon report,) by adding a little soda and potash to a mixture of lime and clay, which I then burned in order to obtain an artificial hydraulic lime.

Mr. Pitot du Helles, formerly an officer of Engineers, now living at Morlaix, sent me, lately, a singular substance which furnishes hydraulic lime, and seems to me to bear analogy to the Boulogne pebbles. This substance appears to come from the fragments of a species of madrepore of which the geological position does not seem to be well known, but which appears to exist outside the roadstead of Morlaix: these fragments are brought by the currents, and form considerable deposits in the roadstead, whence they are raised with the drag, by the boatmen: the farmers make considerable use of it as a manure for certain soils, and give it the name of *merle*. It is in very small and contorted fragments; its colour is a dirty white; it is light. Mr. Grimm, *pharmacieur en chef de la Marine*, at Brest, made an analysis of this substance, which contains: carbonate of lime, 5.25; sand, 1.35; water and organic matter, 3.30; oxide of iron and phosphate of lime, traces; loss, 0.10. Mr. Pitot made lime of this substance, and I made some trials with the specimen he sent me.

When water was thrown on the calcined *merle*, it was almost a quarter of an hour before any heat was manifested, and that so slight that a little vapour was barely perceptible. This lime does not reduce well to powder by slaking; it would be necessary to reduce it by pestles: in this, it presents similar character to the Boulogne pebbles.

I reduced a portion of this lime to paste with water, and placed it at the bottom of a glass filled with water: the hardening was complete at the end of six days, although the experiment was made towards the close of November. I mixed a portion of this lime with an equal quantity of sand: the hardening took place in eight days; and it seemed to me that this lime would not bear much sand. I was not able to ascertain the tenacity of mortar made of this lime; but the manner in which it hardened, and the degree of induration it had acquired in a short time, led me to think that it is a very good hydraulic lime; and that it will be of great advantage in all hydraulic constructions on the coast of Bretagne. I am astonished that alumine was not found in this lime, so eminently hydraulic.

(TO BE CONTINUED.)

Physical Science.

Electro-Magnetic Telegraph.

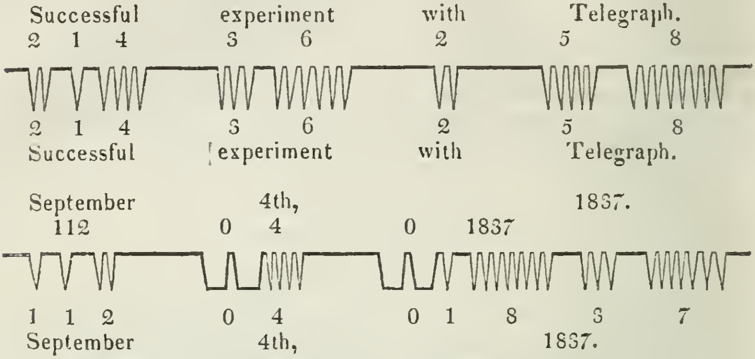
On a recent visit to New York, the Editor had an opportunity of examining the apparatus constructed by Professor Morse, of the University of that city. The subjoined account of it from Prof. Silliman's Journal for the present month (October) will afford a correct idea of the manner in which the communications made by it are recorded, and read off. Our examination of it was subsequent to the addition of the thousand feet of wire, and, although in an experimental essay of this kind we do not look for a perfect arrangement of the instrument employed, yet its operation might be pronounced perfect, and no reason was perceived why its indications may not be given at greater distances than by the ordinary telegraph, whilst they will be uninfluenced by the causes which limit the application of the latter to day-light, and clear weather.

1. *Morse's Electro-Magnetic Telegraph.*—While a contest is waging in several contries of Europe—in England, Scotland, France and Germany, for the discovery and invention of the Electric Telegraph, it may not be amiss to state, that America also claims to be an independent discoverer and claimant for priority in the invention. The dates, the names of the inventors, and other circumstances will doutless ere long be published, and then the world can judge between the conflicting parties. In the mean time it is well ascertained, that Prof. Morse, of the New York City University, conceived and planned, five years ago, an electric Telegraph, while on his passage home from France, and immediately on his landing, he commenced the machinery. Early last spring, *in April*, the general features of his plan were very extensively published in the newspapers, and very lately, *in August*, we learn that several telegraphs on the basis of electricity are in various stages of progress in Europe.

The distinguishing features of Prof. Morse's telegraph are a *Register*, which permanently records in characters easily legible the fullest communication, and the use of but *one wire* as a conductor; although for greater convenience of communicating at all times, and of having a whole circuit at command from each extremity of the line he will use four wires.

On Sept. 2d, Prof. Morse tried an experiment with a circuit of copper wire one thousand seven hundred feet in length, and of the minimum size of No. 18 wire. The record of the Register was sufficiently perfect to demonstrate the practicability of the plan. On the 4th of September some slight changes were made in the machinery, when the Register recorded perfectly the following signs:

Specimen of Telegraphic writing made by means of electricity at the distance of one-third of a mile.



The *words* in the diagram were the intelligence transmitted.

The *numbers*, (in this instance arbitrary,) are the numbers of the words in a Telegraphic dictionary.

The *points* are the markings of the Register, each point being marked every time the electric fluid passes.

The Register marks but one kind of mark, to wit, (V). This can be varied two ways. By intervals thus (V VV VVV) signifying one, two, three, &c., and by reversing thus (Λ); examples of both these varieties are seen in the diagram.

The single numbers are separated by *short*, and the whole numbers by *long intervals.*

To illustrate by the diagram, the word "successful" is first found in the dictionary, and its telegraphic number 214 is set up in a species of type prepared for the purpose, and so of the other words. The types then operate upon the machinery and serve to regulate the times and intervals of the passages of electricity. Each passage of the fluid causes a pencil at the extremity of the wire to mark the points as in the diagram.

To read the marks, count the points at the bottom of each line. It will be perceived that two points come first, separated by a *short* interval from the next point. Set 2 beneath it. Then comes one point likewise separated by a *short* interval. Set 1 beneath it. Then come four points. Set 4 beneath it. But the interval in this case is a *long* interval, consequently the three numbers comprise the whole number 214.

So proceed with the rest until the numbers are all set down. Then by referring to the Telegraphic Dictionary, the words corresponding to the numbers are found, and the communication read. Thus it will be seen that by means of the changes upon *ten* characters, all words can be transmitted. But there are *two points* reversed in the lower line. These are the *eleventh* character, placed before a number to signify that it is to be read as a *number*, and not as the representative of a word.

Since the 4th of September, one thousand feet more of wire No. 23 have been added, making in all two thousand seven hundred feet—more than half a mile of a reduced size of wire; the Register still recorded accurately.

Arrangements have been made for establishing a circuit of several miles, and for constructing new and accurate machinery. Prof. Gale, of the New York City University is engaged with Prof. Morse in making some interesting experiments connected with this invention, and to test the effect of length of wire on the magnetizing influence of voltaic electricity.

To the foregoing notice, we append an article published in Thomson's Annals of Philosophy, Vol. VII, p. 162, first series, February 1816. This article is from the pen of Dr. John Redman Coxe, of Philadelphia; and it is believed that the idea of the employment of Galvanism, as a Telegraph, which it suggests, was then original. Those who are acquainted with the history of the progress of Electricity, as evolved by the ordinary machine, are aware that experiments had been made with a view to its employment for a similar purpose; but from the inherent difficulties of the subject, the project had been abandoned.

It is not pretended that the state of our knowledge, on the subject of Galvanism, was such at the time the foregoing suggestion was made, as would have enabled any person to apply it practically, this, if done, will be due to the recent discoveries on the subject of Electro Magnetism, a subject which has been very successfully pursued by the Philosophers of our own country, and particularly by Professor Henry, of Princeton. As some of the Philosophers of Europe, are disputing upon the question of the authorship of the proposition for the employment of Galvanic Electricity, Telegraphically, we have thought that it would not be altogether inopportune, or uninteresting, to republish the article above referred to.

"*Use of Galvanism as a Telegraph; in an extract of a letter from Dr. J. Redman Coxe, Professor of Chemistry, Philadelphia.*

"I observe in one of the volumes of your annals of philosophy, a proposition to employ Galvanism, as a Solvent for the urinary calculus, but which has been very properly, I think, opposed by Mr. Armiger. I merely notice this, as it gives me the opportunity of saying, that a similar idea was maintained in a thesis, *three years* ago, by a graduate of the University of Pennsylvania.

"I have, however, contemplated this important agent, as a probable means of establishing Telegraphic communications, with as much rapidity, and perhaps less expense, than any hitherto employed. I do not know how far experiment has determined Galvanic action, to be communicated by means of wires; but there is no reason to suppose it confined, as to limits, certainly not as to time. Now by means of apparatus, fixed at certain distances, as telegraphic stations; by tubes for the decomposition of water and of metallic salts, &c. regularly ranged, such a key might be adopted as would be requisite to communicate words, sentences, or figures, from one station to another, and so on to the end of the line. I will take another opportunity, to enlarge upon this, as I think it might serve many useful purposes; but, like all others, it requires time to mature. As it takes up little room, and may be fixed in private, it might in many cases, of besieged towns, &c. convey useful intelligence, with scarcely a chance of detection, by the enemy. However fanciful in speculation, I have no doubt that sooner or later, it will be rendered useful in practice.

"I have thus, my dear sir, ventured to encroach upon your time, with some crude ideas, that may serve to elicit some useful experiments in the hands of others. When we consider what wonderful results have arisen from the first trifling experiments, of the junction of a small piece of silver and zinc in so short a period, what may not be expected from the further extension of galvanic electricity! I have no doubt of its being the chiefest agent in the hands of nature, of the mighty changes that occur around us. If the metals are compound bodies, which I doubt not, will not this active principle combine those constituents in numerous places, so as to explain their metallic formation? and if such constituents are in themselves aeriform, may not galvanism reasonably tend to explain the existence of metals in situations to which their specific gravities, certainly do not entitle us to look for them?

Bibliographical Notice.

Elementary principles of carpentry, by Thomas Tredgold, Civil Engineer; first American, from the second London Edition, corrected and considerably enlarged. E. L. Carey and A. Hart, Philadelphia, 1837.

This work forms an excellent elementary Manual, for the carpenter, as well as a valuable book of reference, for the Architect, or the Engineer.

The principles laid down by Mr. Tredgold, relating to that branch of the art, which is particularly directed to the support of weight or pressure, are all founded upon the immutable laws of mechanics, and explained with great perspicuity:—The strength of various kinds of timber to resist *tension*, *compression*, or *cross strains*, is fully investigated, and copious tables are given, exhibiting the results of experiments, on the strength and cohesion of almost every description of wood.

One of the most important divisions of the work, is that which is devoted to the investigation of the *nature and properties of timber.* Many very curious and interesting facts, connected with the growth of trees, are mentioned, and much valuable information is furnished on the subject of felling and seasoning timber, the prevention of decay and dry rot, and the durability of wood in different situations.

The tables of scantlings for roofs, trusses and girders of various span, are also of great utility to practical men. These tables have, however, been prepared for the yellow fir of Europe; it will, therefore, be necessary, in adapting them to other woods, to have reference to the strength of the material to be used, in comparison with the strength of yellow fir; these calculations are rendered easy by the tables giving the *properties* and the *specific gravities* of timber; which tables (by the way) are by no means the least useful portion of the work.

A table of the specific gravities of other substances, forms also a part of this valuable production; and the whole work is illustrated by engravings, executed in a very superior style.

Mechanics' Register.

THE PATENT OFFICE.

The notices of Patents, granted by the United States, have been omitted since the destruction of the Patent Office, with all its Records and Models, on the 15th of December last; these notices we now resume, and on doing this, the Editor offers the following remarks, and explanations, upon certain points, which appear to him to be of interest to those in any way concerned in patents for useful inventions.

It has long been the practice of the Editor, to delay a particular notice of patented inventions, for several months after the time of their being granted; a course necessary from various considerations, but more particularly for the two following: First. Patents are not granted in foreign countries for inventions, descriptions of which have been previously published; the arrival of this Journal in England, or in France, one day before the sealing of a patent, would destroy the grant, and cause the loss of the large amount of fees paid into the offices. In applying for such patents, there is usually much delay from the want of proper information; and a considerable length of time must necessarily elapse, in the foreign offices, between the times of application for, and that of the sealing a patent. This alone, would be a sufficient reason for the delay; but there is a second of some personal importance, to the Editor himself, as well as to patentees. It is the practice of the Editor, to give his own opinions, freely and fully, respecting the value of inventions, and it is his wish, not to interfere with the interests of the inventor, by forestaling public opinion, before he shall have had time to test the value of his supposed improvements, by a practical test.

Under the operation of the acts of July 4th, 1836, and of the 3rd of March, 1837, by which all former laws relating to patents, were repealed, all applications for patents are subjected to a critical examination in the Patent Office, and if the things claimed, are known to be old, or if that which is proposed to be done, contravenes the established laws of Mechanical and Chemical Philosophy, the patent is refused; subject, however, to an appeal from the decision of the office. Under this law, there are two examiners appointed, of whom the Editor is one, and it will, therefore, be manifest that the tone of his animadvertions, must be modified by the existence of these circumstances. Under the official examination, a large class of applications, will be rejected, and never, therefore, meet the public eye; this will be a source of sufficient mortification to those who have dreamed of reaping a golden harvest, without the lash of criticism. It must not be inferred from these remarks, that our list of patents will become a mere detail of approved inventions; the fact will be far otherwise; for although the office is now possessed of judicial power to a certain extent, and can arrest what is old, what is contrary to the laws of nature, and what is deemed altogether trifling, it would be stepping out of the line of its duty, and transcending its legitimate powers, to refuse a patent for an alleged improvement or invention, because the Examiners, or the Commissioner, may believe the thing worthless. They are not to act under the guidance of mere opinion, but must, and ought to, give the sanction of the office in all cases where there is apparent novelty. The utility is a question to be subsequently settled between inventors and the public. Nor is the absolute amount of novelty, a question for this office, as a very large amount of utility, not unfrequently results from a trifling change in the construction of a machine; the duty of the office in this case, is to see that the claim does not embrace more than the invention of the applicant; and, whenever there is a doubt respecting the propriety of rejection, the decision of the office will always be given in favour of the claimant, who, in such case, has an undoubted right to a final determination of his interests, by a Court and Jury.

The number of patents hereafter issued for quack medicines, will, under the operation of the existing law, be much diminished, and, perhaps, it would be for the public benefit, could they be entirely cut off from the sanction of an exclusive right; but this cannot be done in all cases. Where compounds substantially the same with those known to the practicing Physician, are attempted to be patented, they will, of course, be excluded, but they must be granted when the compound appears to possess novelty, and is not manifestly of such a nature as to produce public injury; the Journal, however, will fully make known every recipe for a patent medicine, accompanied by such remarks as may appear just and proper.

The enquiry is frequently made, what proportion of the applications under the present law, are successful? Without taking the trouble to estimate the precise number, it may be stated, that not one third pass as originally presented, a large number being returned for amendment, and modification of the claim; probably about one half of those applied for, are eventually granted.

The next number of the Journal, will contain some information respecting the restoration of the records and models of the office, as it appears that the circular issued upon this subject, has not, generally, been clearly understood.

American Patents.

LIST OF PATENTS WHICH ISSUED IN JANUARY AND FEBRUARY 1837.

With remarks and Exemplifications by the Editor.

1. For improvements in *Reaction Water Wheels*, Gideon Hotchkiss, Windsor, Broome County, New York, January 19.

Patents for reaction wheels, are so numerous, that there does not now appear to be any thing left to claim beyond certain minor peculiarities in individual parts, which, although they may present sufficient novelty to render it proper for the office to grant a patent therefor, are not likely so far to change the character of such wheels, as to justify their adoption where economy of water is an object of any importance, although they may thereby be rendered less objectionable, in those situations where circumstances may render their employment convenient.

According to the improvements, now proposed, there are to be several wheels placed upon one horizontal shaft, which wheels are, in their general construction, exactly like those used by Calvin Wing, and by several others whose plans have been described in this Journal. In the drawing before us, there are four such wheels combined together; and the first claim made, is to a peculiarity, in the form of the opening in the head through which the water is admitted. Instead of being a circular opening, extending to the inner edges of the buckets, it is to be scolloped, there being as many hollow curves as there are buckets, the edges of which curves are to be sharpened, that they may "cut away opposing substances, such as leaves, straw, wood, &c." We apprehend that this claim must stand upon the novelty of the structure, without any aid from its utility. The second claim is to the so casting, or forming, the eye of the wheel, that it may be reversed on the shaft, which will reverse the direction of its motion. This is a small affair, the value of which we leave to the estimation of those concerned. The third claim is to "the manner of constructing the wheel with cast-iron hub and arms." This is the child of the second claim, and consists in the particular mode of forming the parts for the purpose of reversing, as therein noted. The fourth claim is to "the use and application of the combined pivot gates, or valves, for letting on or shutting off the water." This is a claim to the particular arrangement of the levers, for opening or closing the valves or gates, and secures nothing but this peculiar mode of doing a thing, which may be effected in other ways. The sixth claim is to "the making of the wheel case portable, or entirely independent of the mill frame, with the axles of the wheels supported by, and turning in the same." This latter device, would seem to render the second, third, and fourth unnecessary, as by turning the portable case, end for end, the reversing operation would be completely effected.

2. For *Fastenings for Harness;* Warner Hayden, New Milford, Susquehanna County, Pennsylvania, January 21.

The patentee says, that he effects the object proposed, of fastening harness, "by means of metallic clasps, screws, and pins, which I use as substitutes for the buckles, usually employed for that purpose." The improvement is not confined to any particular part of the harness, but the clasps and pins are to be used, both on the traces, and on the smaller straps. The clasps may be made more ornamental than buckles, and are simple in construction, consisting of a flat plate with a staple, or staples upon it, through which the end of a trace, or strap, is to be passed, and in which it is confined by a sufficiently stout screw pin, or pins, passing through the staple and leather strap, and screwing into the plate below. One screw is generally deemed sufficient; but for traces, or other parts where there is much strain, two may be employed.

In some cases it is proposed to have two staples upon a plate, to receive the strap, and between the two staples, to have a pin firmly attached to, and rising from the plate, so that by bending the strap, the pin may enter one of the holes, when, by straightening the strap, the fastening will be completed. The claim is to "the within described manner of making and using such fastenings for harness, wherever buckles have been heretofore employed for that purpose."

We have examined specimens of harness thus fastened, and the plan is certainly recommended both by its beauty and neatness; but we have heard some objections made to it, by practical men, and have not learnt, whether these have been found valid upon actual trial. It has been said that the bend of the leather, over the rim of the buckle, is essential to the sustaining the strain upon the tongue, or pin, without which the leather, or pin, would be likely to give way. It has been apprehended, also, that the screws would be much more liable to come loose, than a buckle, a circumstance calculated to produce very injurious effects.

3. For a "*Saddle for removing the sick and wounded*, and answering also as a pack saddle; Hezekiah L. Thirtle, City of New Orleans, January 21.

A long saddle tree, is to be adapted to the back of a horse, and from each end of this, two springs are to rise, like coach springs, which are to sustain the four corners of an iron cot frame, sufficiently long and wide for a man to lie on, when it has a suitable bottom strained on it; sides and ends of leather are to be raised around the cot frame. Straps are to be attached to the apparatus, to steady and secure the person to be carried. The claim is to "the before described saddle, for conveying the sick and wounded, and answering also as a pack saddle, when not used for the above purpose."

We are told that this contrivance is considered new, by such officers of the army who have examined it, and that it is believed it may be very advantageously employed in the present war in Florida, where, from the nature and state of the country, there are but few roads which can be travelled by wheeled carriages.

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4. For *Transparent Door Plates*; William C. Austin, Greenville, Augusta County, Virginia, January 31.

Plate glass is to be set in a suitable metallic frame, one surface of which glass is to be ground, to render it semi-transparent. Upon this, plate letters of metal are to be affixed, by pins passing through holes drilled in the glass, or in any other suitable way. A hole is to be cut through the door, upon which the plate is to be placed, so that by means of an ordinary entry lamp, the name may be rendered visible at night, whilst the glass will form an agreeable ground for the letters by day.

The Patentee says, "I do not claim the use of door plates formed of transparent plates of glass, lettered on the back, and contained within metallic frames, as of my invention; but what I do claim, is an improvement on such plates, by cutting a hole through the door, and affixing them in such manner as that a light within the passage, or hall, shall, at night, shine through them for the purpose herein set forth."

Names and other devices, have been painted upon glass, both transparent and ground, but probably there may be novelty enough in the proposed plan of affixing ground glass, as door plates, to sustain a patent; and to Physicians there may be utility in their employment, but their extensive adoption is a very doubtful affair.

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5. For improvements in *Cooking Stoves;* Benjamin W. Wood, Bath, Steuben County, New York, January 31.

The grate upon which the fuel rests, in this stove, is made capable of being raised and lowered by means of racks and pinions; there being a rack at each end of the grate, and two pinions on a shaft crossing the front of the stove, and taking into the teeth of the racks. Variations in the manner of effecting this, are pointed out. At the back of the fire-place, there is to be an oven, having a circular plate, forming the principal part of its bottom, which plate is capable of revolving, there being teeth on its under side into which a pinion is geered, for the purpose of turning it. The upper plate of the oven is also circular, and revolves with the lower, whilst it is capable, also, of being raised or lowered, by means pointed out by the patentee.

Instead of such grooves, or edges, as are ordinarily cast on the edges of the plates, to hold them in place, there are dovetail grooves cast on the upper and lower plates, into which the side plates may slide, and become securely fixed without other fastenings; the end plates are similarly fastened.

To cause the back plate of the fire chamber, to become highly heated, so that it may communicate sufficient heat to the oven, there are ribs formed on its surface, to prevent the direct contact of the wood, and allow a free course to the draught and flame.

Claim.—"I do not claim to be the inventor of the raising of a grate, by means of a windlass or other analogous contrivance, this, standing alone, not being new, nor do I claim the mere making of a circular plate, to revolve upon the bottom of an oven, by means of a rack and pinion, this also having been effected, although by other means; but what I do claim, is the combination of the apparatus for raising the furnace grate, with a stove, having a revolving bottom plate to the oven thereof, with the second plate herein described which is capable of being raised and lowered, and of revolving with the bottom plate, by means analogous to those described, and for the purposes herein set forth. I claim also, the manner of putting together of cast iron stoves, of any description, by means of dovetailed tongues and grooves; and the placing of ribs, or ridges, up the back plate of the furnace to admit the flame and heated air, to come into contact therewith."

Whatever apparent utility there may be in the contrivances for adjusting the fire, and the oven, in the above described stove, will be more than counterbalanced by its real complexity. In articles of perpetual use, and under the supervision of such cooks as are said to be the envoys of his satanic majesty, the utmost simplicity of construction is requisite, or derangement of parts, and an additional excuse for spoiling the meat, will be the sure result. The best part of the foregoing contrivance, appears to be the putting of stoves together by dovetailed grooves, instead of by rods and screw-nuts.

6. For a *Machine for Boreing and Mortising,* John H. Power, Norwalk, Huron County, New York, January 31.

This machine, is principally intended for boreing and mortising holes in posts, for post and rail fence. It consists mainly of three screw augers, placed side by side, in a frame, with pinions on their shanks, so geared that by turning a crank, the three augers will revolve, the middle one in a direction the reverse of the other two. To cut the three holes into one, there are four chisels, which advance with the frames, and are so situated that they pare off the redundant stuff between the holes. The middle, or left hand auger, has two projections on it, which are claimed, but the object for which they are formed, is not very apparent. The combination of the augers and chisels, is also claimed.

7. For an improvement in the construction of *Elliptical, or double Bow Springs, for Carriages;* William Croasdale, Hartsville, Bucks County, Pennsylvania, February 3. (See specification.)

8. For a *Washing Machine*; William Hovey, Worcester, Worcester County, Massachusetts, February 4.

This machine in its general construction, does not differ materially from some of the host of washing machines, previously patented. A roller ten or twelve inches in diameter is to revolve in the trough, which is to contain soapsuds, and the clothes to be washed. This main roller is fluted from end to end, and is made to revolve in the trough, by means of a winch. Beneath this main roller, there is to be a hollow segment surrounding about five-twelfths of its surface, and within this segment, there are to be rollers of about an inch in diameter, which turn upon gudgeons, and are so placed as to correspond with the flutes on the main roller. This segment is to be borne up against the main roller by spiral springs, and to enable it to adapt itself to the main roller, it is divided into two parts, meeting each other along the bottom. A very elaborate description is given of the particular construction of the divided segment, its slides and springs, as these, but principally the divided segment, are the subjects of the claim. The clothes are to be washed, by passing and repassing between the main roller and the segment of rollers.

There is not any thing in this machine, to insure it against the fate which has awaited the whole tribe of washing machines, that of sinking into an early grave, unwept, excepting by their immediate parents, whose regrets will not unfrequently be accompanied by a wish, that the child had never been born.

9. For an improvement in a *Machine for Weighing Heavy Bodies;* Erastus and Thaddeus Fairbanks, St. Johnsbury, Caledonia County, Vermont, February 10.

A patent under a similar title, was obtained by the Messrs. Fairbanks, on the 13th of June, 1831, which patent was surrendered and reissued under an amended specification, on the 6th of March, 1834; this last has also been surrendered, and that before us issued in its stead. Other patents were also obtained for improvements on their weighing apparatus, by the same gentlemen, one of which was dated in February, and another in September, 1832. These also have been surrendered, and in lieu of them, seven new patents have been obtained; the main object of the Patentees, in adopting this course, it is believed, has been to separate their several improvements from each other, conceiving that in case of infringement of their rights by the adoption of certain parts of them, there would be an advantage, in prosecuting a suit, upon a patent unincumbered by improvements not in question. The former specifications have been noticed, and one of them inserted in full, in this Journal, soon after they were granted, we shall, therefore, give but little more, than the claims under the present specifications. The claim in the first of the series, is to the knife-edge hinges, constructed in the manner herein set forth, and the pins denominated rockers, as shown in the drawing."

10. The title and date of this patent, are the same with the foregoing, and the subject matter of it was contained in the original patents, surrendered as above. The following is the claim.

"We do not claim the invention of knife edge bearings, but we do claim the employment of two levers, connected together at a point equidistant from their axes, or at the point of their relative power, in combination with two pairs of knife edge hinges, or bearings, whose knife edges are parallel lines."

11. For improvements in the mode of constructing, and *Weighing by, the Steelyard Balance;* re-issued on the surrender of the Patent of September 22, 1832, February 10.

Claim.—"We do not claim the movable weight attached to the beam, for the purpose *simply* of obtaining an exact counterpoise to the platform, as heretofore used; but we do claim it in the positions above described, for the double purpose of obtaining an exact counterpoise to the platform, *and* of putting the beam into equilibrium."

The original specification of this patent will be found at p. 399, vol. XI., of this Journal, with a cut of the apparatus. We are apprehensive that the claim now made will not be found to obviate the objections to which the former may have been liable, or, at all events, that the form given to it is itself vulnerable. Patents are not granted for results, but for the means by which the desired results are attained; the decisions of the courts are clear upon this point. The foregoing claim is manifestly to the result obtained by the manner of attaching the movable weight; and the terms employed would seem to indicate that such weights, so attached, had been formerly employed for the purpose of balancing the platform only, but that the patentees had discovered that the same weight served also to put the beam into equilibrium, and claim a patent for this discovery. This, however, is not the kind of discovery for which patents are granted. We have always believed, and still believe, that the particular manner of attaching the movable weight, described in the specification, was the invention of the patentees, and if so, the results, whatever they may be, belong to them, because they have a right to the means by which they are obtained.

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12. For improvements in the mode of constructing, and *Weighing by the Steelyard Balance;* re-issued on the surrender of the Patent of September 22, 1832, February 10.

Claim—" What we claim, and that for which we ask a patent, is the combination of the use of the graduated weights as above described, to be suspended at the end of the beam, indicating the hundreds and thousands of pounds, with a movable poise traversing the arm of the beam, and showing the intermediate pounds."

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13. Improvement in the *Machine for Weighing Heavy Bodies;* Patent of February 21st, 1832, surrendered, and the present re-issued February 10.

Claim—" What we claim as our invention, and as our improvement upon the machine formerly patented by us, is the variation in the manner of arranging the levers as described. The arrangement being that of having the longer lever, with its rocker, triangular, extending through the centre of the machine; and the shorter lever, while it forms but one power, is constructed with two arms, so fitted as to be suspended to the longer lever at a point equidistant from the axis of each, thus preserving the simple combination of two levers, as claimed in our former patent."

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14. Improvement in the *Machine for Weighing Heavy Bodies;* Patent of February 21st, 1832, surrendered, and the present re-issued, February 10.

Claim.—" What we claim as an improvement upon the weighing machine formerly patented by us, is the mode of obtaining a vibrating motion in the supporting power by means of the standards (E E) which are used instead of the fulcrum used in our first machine."

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15. Improvement in the *Machine for Weighing Heavy Bodies;* Patent of February 21st, 1832, surrendered, and the present re-issued, February 10.

Claim.—"What we claim as new, and as an improvement on the machine formerly patented by us, is the variation of the manner of connecting the two levers in the centre, (whether they be composed of one or more parts each) to wit, by means of knife edges resting in pendulous rings, or circles."

16. For a composition for *rendering Boots and Shoes Water-proof;* Patrick G. Nagle, City of Philadelphia, Feb. 10. (See Specification.)

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17. For improvements in the *Knitting Machine;* for which Letters Patent were granted on the 5th day of March, 1831, John McMullen, Sinking Valley, and Joseph Hollen, Jr., Huntingdon county, Pennsylvania, February 11.

It will be readily believed that a machine for knitting stockings must be one of cosiderable complexity, and that a correct idea of its individual parts cannot be given in words only. Even with the drawings it would be quite a study to trace the action of that before us. We shall not, therefore, attempt a description, nor shall we copy the claim, but merely observe that the machine is what it purports to be, a real improvement upon that formerly patented. We have spoken of it as complex, but we do not think it unnecessarily so; it performs its work well, does not appear liable to derangement with ordinary care, requires but little address in its management, and reflects a very high degree of credit upon the inventors, who have manifested a rare degree of skill in the whole arrangement.

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18. For improvements in the *mode of manufacturing Wooden Screw Bedsteads;* Palmer Williams, Towanda, Bradford county, Pennsylvania, February 16.

The bedstead rails are to have right and left handed screws on opposite ends, and corresponding female screws are to be cut in the posts; but the patent is obtained for the machinery by means of which the screws are so cut that they will come correctly to a shoulder at each end, when they are screwed into the posts. The apparatus for cutting the screws does not differ from that known to workmen, and the machinery employed merely confines the posts whilst being cut, so that the proposed effect will be produced.

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19. For an improvement in the manner of constructing *Bench and other Vices;* Linus Dean, Utica, Oneida county, New York, February 16. (See Specification.)

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20. For an improved mode of *Holding back a sled on icy descents;* James Andrews, Camden, Waldo county, Maine, February 16.

On the back end of each runner of the sled there is to be a hinged clip, which embraces the runner on each side, and has on its back end a kooked point, bending downwards, which is capable of being forced into the ice when descending a hill ; for the purpose of forcing it down there is a screw above each of the hold-backs, which may be turned at pleasure, and made to act upon the clip. The claim is to "the whole of the aforesaid irons, fastened and used as aforesaid."

We apprehend that the foregoing patent will be of little value, as the devices for holding back vehicles upon runners, when descending hills covered with ice are no novelties, and may be used in various ways without interfering with the particular plan above described.

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21. For an improved *Balance for Weighing;* B. Morrison, Milton, Northumberland county, Pennsylvania, February 16.

The apparatus is denominated "The Druggists' Balance," and is a modification of that kind which has the scale above the beam, many of which were imported from England a few years since, having been patented in that country. Mr. Morrison obtained a patent for a balance somewhat similar to this in February 1833, and also in December of that year. (See Journal vol. XII,. p. 93, and vol. XIV., p. 30.) The present plan differs from the former in some points which appear to possess decided advantages, but it is not thought necessary to give a long verbal description of an arrangement which could not be rendered clear without a drawing.

22. For improvements in the process of *Preserving Timber for various purposes;* Webster Flockton of Great Britain; residing in Spa road Bermondsey, Surry county, Near London, February 16. (See Specification.)

23. For a *Machine for Cutting Furs from Skins;* Samuel Johnson, Walnut Lane, Surry county, North Carolina, February 24.

The cutting in this machine is effected by shears resembling tinmen's shears, with the fulcrum at one end; these are made to vibrate by means of a crank and pitman. The skin from which the fur is to be cut passes around and between feeding rollers, which present it to the shears. The claim is to "the method of vibrating the upper shear on a pivot at one end, and regulating the same by the springs bearing against the pitman, as described, and the board for preventing the skin from rising, in combination with each other, and with the other parts of the machine as described."

There does not appear to be much novelty in the plan of the foregoing machine, but the patentee has, in his claims, limited himself to certain particular combinations, which are deemed new.

24. For *Propelling Machinery by means of Electro-Magnetism;* Thomas Davenport, Brandon, Rutland county, Vermont, February 25. (See Specification.)

SPECIFICATIONS OF AMERICAN PATENTS.

Specification of a Patent for Combined Elliptical and Spiral Springs; granted to WILLIAM CROASDALE, *Hartsville, Bucks county, Pennsylvania, February 3d,* 1837.

To all whom it may concern, be it known, that I, William Croasdale, of Hartsville, Bucks county, Pennsylvania, have invented an improvement in the construction of the Eliptical, or double bow springs for carriages of various kinds, and I do hereby declare that the following is a full and exact description thereof.

The springs are made, and put together in the usual manner, my improvement consisting of a spiral spring, or springs, which I interpose between the two bows of the elliptical spring, and sometimes between the ends of the elliptical springs and the axle, and between them and the under side of the carriage. In most cases I deem a single spiral spring sufficient, and this I make of steel wire, of from one-eighth to three-eighths of an inch in diameter, twisted with the coils open, and placed in the middle between the two bows, so as to extend from one to the other; the ends of the wire entering into holes prepared to receive them.

I generally wind these springs with the coils each of the same size, when the outline will be cylindrical, but sometimes I wind them so that their outline may be that of a double cone joined at their smaller ends, and the coils then pass more or less the one within the other, when pressed closely together. The outline may also be conical, or the spring may be bent in a zig-zag form, or otherwise, but I deem the spiral best.

If preferred, there may be spiral springs placed under the elliptical springs, their lower ends bearing on the axle-tree; and sometimes also above the ends of the elliptical springs, extending up to, and bearing against, and a suitable piece of timber attached to the under side of the carriage, or vehicle; thus there may be four such springs without, whilst there may be two or more such springs placed at suitable distances apart within, the elliptical springs, so graduated as to adapt them to the double bow. The spiral springs thus placed, give greater elasticity and liveliness to the elliptical spring, and also tend to prevent its breaking.

What I claim as my invention, and wish to secure by Letters Patent, is the adding to, and combining with, the elliptical, or double bow springs, one or more spiral springs, in the manner and for the purpose herein set forth. William Croasdale.

Specification of a Patent for rendering Boots and Shoes Water-proof; granted to Patrick G. Nagle, *of the City of Philadelphia, February* 10*th*, 1837.

Be it known that I, Patrick G. Nagle, of Philadelphia, in the County of Philadelphia, and State of Pennsylvania, have discovered a new and useful composition for rendering boots and shoes water-proof, which is described as follows.

Take two pounds of balsam of capivi, five pounds of the essence of the myrtle tree, one pound of gum copal, two pounds of rosin, three pounds of rendered suet; cut all the hard subtances into small particles. Put the whole into a large vessel, and let the admixture boil for a few minutes until the ingredients become well incorporated together, and converted into a paste.

Mode of Application.—When the boots are crimped, and while wet on the crimping-boards, lay on the paste (it being warm at the time of applying the same;) in this state it will penetrate every part of the leather as it dries.

The above is for the leg part of the boot.

The following is for the bottom part of the boot.

Take the above in a dissolved state, or a part of it, and apply a sufficient quantity of rosin so as to make it into a thick paste (the rosin to be melted.) Then lay it on in the usual way in which the workman puts on paste after sowing on the welt. In this manner the inner sole becomes prepared to resist water as well as the outer sole.

The discovery claimed by the subscriber, and by him desired to be secured by Letters Patent, consists in the before described composition for rendering boots and shoes water-proof. Patrick G. Nagle.

Remarks by the Editor.—Numerous mixtures of wax, resins, and fatty matters have been employed for the purpose of rendering leather water-proof, and recipes of this kind are common in the journals, and other works In that given by the patentee there is some obscurity, and some empiricism

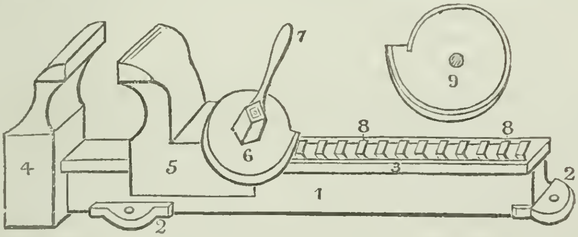
We do not know what is intended by "essence of the myrtle tree," but suppose that myrtle wax may be the thing designed; if so, it is a novel kind of essence. We apprehend that common turpentine would answer all the purposes intended to be answered by the capivi, (copaivi) and that in actual practice it is substituted.

The directions for saturating the sole are rather obscure; what is intended by "taking the above in a dissolved state, or a part of it," not being very apparent. The patentee has an undoubted right to the composition which he has claimed, but to this he is confined, and if it is superior to any of the analogous mixtures which have been used, or which may be readily made without infringing his rights, he has a better thing than we believe he has.

Specification of a Patent for an improvement in the manner of constructing Bench, and other Vices; granted to Linus Dean, *City of Utica, State of New York, February 16th,* 1837.

To all whom it may concern: be it known, that I, Linus Dean, of the City of Utica, in the county of Oneida, and State of New York, have invented a new and improved mode of constructing Bench, and other Vices; and I do hereby declare that the following is a full and exact description thereof.

In my improved vice, the movable jaw is not made to open by means of a joint pin, in the ordinary manner, but it is provided with grooves on its lower side, which fit on to, and by means of which it slides freely along a horizontal bar, the upper edge of which projects over on each side so as to form tongues, or ribs, which fit into the grooves of the sliding jaw. Upon one end of this horizontal bar the fixed jaw is situated, being firmly attached thereto. The bar has flanches on its lower side, by which it is to be attached to the bench. Instead of moving the sliding jaw by a screw, I effect this by means of a rack and a spiral, or volute, wheel, the fillet of which takes into the teeth on the rack, which teeth occupy the upper side of the sliding bar.



1, Bar; 2, flanches; 3, tongue; 4, fixed jaw; 5, sliding jaw; 6, volute wheel; 7, handle; 8, rack; 9, inside of volute wheel.

The spiral, or volute wheel revolves upon a pin rising from the sliding jaw, near to its back end; the spiral, or volute, is formed upon the lower face of this wheel, making one entire turn thereon. The whole is inclined back from a horizontal position, to enable the fillet, or spiral, to take into the rack teeth; a projecting pintle on the upper side of the wheel receives a handle, or wrench, by which it may be turned, which pintle forms a socket in the centre of the wheel to receive the pin on which it turns. The size of the respective parts will, of course, depend upon the size of the vice itself. The following are the general dimensions of the principal parts of one which I have made. The bar is about two feet in length, two and a half inches deep; and one and a fourth inch wide. The fixed jaw is about the size, and is in the form of, that of an ordinary smith's vice; the movable jaw is of the same size and form, but it has a projecting piece extending back about six inches, which forms a part of the cheeks of the slide, and from which rises the pin upon which the spiral wheel revolves; the wheel is about six inches in diameter, the thread, or fillet, forming the spiral, about five-eighths of an inch square; and the pin which passes through the wheel, two inches high, and one inch and a half in diameter. The bar of this vice is to be secured upon the bench, with the fixed jaw towards the workman.

Having thus fully described the construction of my improved vice, I do hereby declare that I do not claim to be the inventor of the sliding jaw, this having been before made and used, but it has hitherto been moved, as in the common vice, by means of a screw. What I claim, therefore, as constituting my invention and improvement, is the substituting the rack and spiral wheel for the screw, as herein set forth, and arranging the auxiliary parts substantially in the manner described. Linus Dean.

Remarks by the Editor.—When we first examined the description and drawing of this vice, we were impressed with the idea, that the mode of opening and closing it would prove inconvenient if not otherwise objectionable, but we have since seen and carefully examined a number of them with much satisfaction. The spiral wheel operates not only well, but pleasantly. The extent to which the jaws may be separated is very great, and in all situations they preserve their parallelism, the convenience of which is at once apparent. By raising the spiral wheel the sliding jaw may be slid along, and much time in opening and closing be thus saved. In fine, we think the contrivance not only possesses the merit of originality, but the more important one of real utility, for many purposes.

Specification of a Patent for improvements in the mode of Preserving Timber for various purposes; granted to Webster Flockton, *of the kingdom of Great Britain, February* 16*th*, 1837.

To all whom it may concern: be it known, that I, Webster Flockton, of Bermondsey, in the county of Surrey, in the kingdom of Great Britain, Merchant, have invented improvements in preserving Timber, or Wood, for various purposes. My invention consists in impregnating timber, or wood of various descriptions, with a metallic solution, whereby such timber or wood will be preserved: and in order to produce the necessary solution, I saturate the essential oil of vegetable tar with the oxide of iron, which I consider the best and cheapest means of carrying my invention into effect, though I do not confine myself to the precise means hereafter described, as variations may be made; my object being to impregnate timber, or wood, for any purpose to which it may be found applicable, with a metallic oxide as above stated and as is more fully described hereafter.

But in order that my invention may be fully understood and carried into effect, I will describe the method I pursue of combining such materials together and applying them to the purposes aforesaid. I take a quantity of tar (either Stockholm, Archangel, or American) which I submit to the process of distillation, and the apparatus, or still, which I use for this purpose is similar to what is called a pitch still, which is made of copper, and well known, and forms no part of my invention, nor does the process of distillation for separating the essential oil from tar, which is effected in the manner following. The still which I use will contain about 400 gallons, but I do not put into it more than three-quarters of that quantity of tar, or twelve barrels, of the usual size, of either of the kinds before mentioned. The first product will be the acid of the tar, bringing with it a light-coloured essential oil which separates immediately and floats upon the surface of the acid in the receiver, which I *prefer of wood* (a cask with one head furnished with a cock for the withdrawing the acid from below, being applicable to the purpose;) after some time the acid will cease and the essential oil will come over in a very considerable stream which I collect from the receiver to the extent of about four gallons to the barrel, or forty-eight gallons in the whole, including that which came over in the first instance with the acid; the fire is then to be withdrawn and the contents of the still, which by the extraction of the essential oil has become *pitch*, allowed to remain *in the still* until the following morning to cool, then it may be let off by means of a pipe fitted with a brass or iron plug, into a large receiver of cast-iron, or other suitable material, and finally put into casks for sale.

I will now proceed to describe the combining of the essential oil with the other materials for the making of my "Metallic Solution." To effect this, I place *two*, or more, large casks upright, removing the upper head of each, and throw into them well rusted iron hoops, or tin cuttings. I then pump into them one hundred gallons, or more, of the essential oil of tar, before described, completely covering the metal. This oil I cause to be repeatedly pumped every day from one cask to the other for about six weeks, by which time the oil will have become very black and much increased in gravity, whilst the iron hoops, or tin cuttings will appear quite bright and free from oxide.

They are then to be taken out and piled up in an open space of ground and set fire to, for the purpose of burning off the oil, and afterwards laid by for re-oxidation, which may be much facilitated by pouring over them a weak solution of common salt and water; when they have again become rusted they are fit for use.

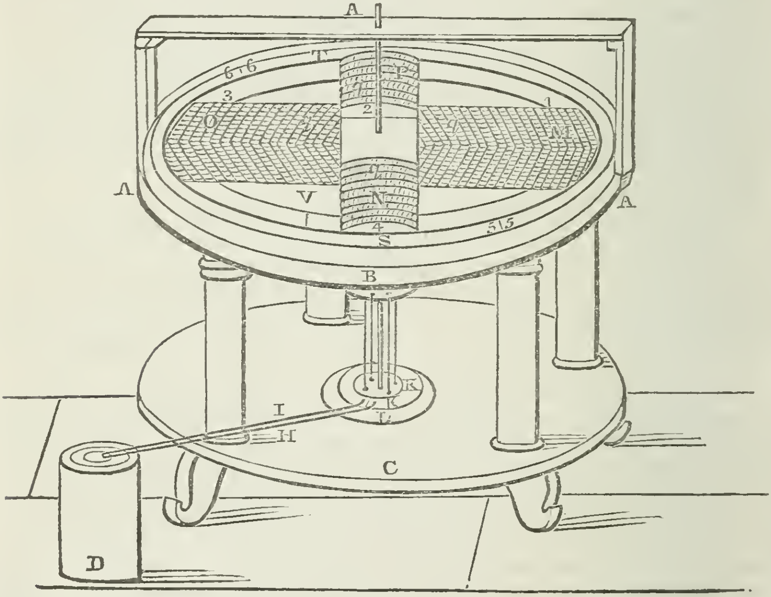
I will now proceed to describe the method I pursue in saturating timber and wood with the metallic oxide.

For saturating piles *already* driven into the sea, forming jetties, or piers, I cause an inch auger to be passed down the centre of the piles to the bottom end, if possible, or as far down as can conveniently be done, and the "liquid oxide" poured down the hole until filled. This is to be repeated as often as may be thought necessary, but, generally, in two or three days it will be found oozing through the pores of the wood, depositing an incrustation of *Iron*, which, in combination with the *Essential Oil* of the *Tar*, resists alike the action of the water and the attacks of the worm. A wooden plug or tree nail is then to be driven fast into the hole, which may be removed by the auger at any time for the purpose of giving the piles a fresh supply. This method is likewise applicable to the timber used in blocking streets, the wood work of railways, and to all wood subject to damp, or moisture, or the attacks of worms, or other vermin.

For out-door buildings liable to dry rot, it may be used cold in the usual way of varnish, or tar, with a brush; for being perfectly liquid it penetrates most rapidly, drying completely in eight or ten hours, when a second dose may be given. Paint applied afterwards dries quickly, but for most purposes two or three applications of the preparation render any other coating unnecessary; for as soon as the pores of the wood become filled, it assumes the appearance of varnish. Having thus decribed the nature of my invention, and the manner of carrying the same into effect, I would have it understood that I lay no claim to any of the materials *separately;* and it will be evident that the means of carrying the same into effect may be varied to suit the particular object to which the invention may be applied: but I would have it understood that what I claim is the impregnating timber, or wood of various descriptions, with the metallic solution above described, whereby such timber or wood will be preserved. WEBSTER FLOCKTON.

Specification of a Patent for the application of Electro Magnetism to the propelling of Machinery; granted to Thomas Davenport, of Brandon, Rutland County, Vermont, February 1837.

Be it known, that I, Thomas Davenport, of the Town of Brandon, in the County of Rutland, and State of Vermont, have made a discovery, being an application of magnetism, and electro-magnetism, for propelling machinery, which is described as follows, reference being had to the annexed drawings of the same, making part of this specification.



The machine for applying the power of magnetism, and electro-magnetism, is described as follows: The frame A may be made of a circular, or any other figure, divided into two, or more platforms, B and C; upon which the apparatus rests, and of a size and strength adapted for the purpose intended. The galvanic battery, D, is constructed by placing plates of copper and zinc, alternately, of any figure, in a vessel of diluted acid: there are two conductors, H and I, one from the copper and one from the zinc, in the vessel D, leading to, and in contact with copper plates, K and L, placed upon the lower platform. These plates, or conductors, are made in the form of a segment of a circle, corresponding in number with the artificial magnets hereinafter described; they are placed around the shaft, detached from one another and from the shaft, having a conductor, leading from the copper plate of the battery, to one of said plates on the lower platform, and another conductor leading from the zinc plate of the battery, to the next plate on said lower platform, and so on alternately (if there be more than two plates on said lower platform) around the circle.

The galvanic magnets, M, N, O, P, are constructed of arms, or pieces, of soft iron in the shape of a straight bar, horse shoe, or any other figure, wound with copper wire *q*, first insulated with silk between the coils: these arms project on lines from the centre of a vertical shaft R, turning on a pivot, or point, in the lower platform; said copper wires, *q*, *q*, extending from the arms parallel, or nearly so, with the shaft down to the copper plates, K and L, and in contact with them.

The galvanic magnets, are fixed on a horizontal wheel of wood V, attached to the shaft.

The artificial Magnets S, T, are made of steel, and in the usual manner. They may be of any number, and degree of strength, and fixed on the upper platform, being segments of nearly the same circle as this platform; or if galvanic magnets are used, (which may be done,) they may be made in the form of a crescent, or horse-shoe, with their poles pointing to the shaft.

Having arranged these artificial magnets, on the top of the upper circular platform, there will be a corresponding number of magnetic poles—the north marked 5, and the South pole 6. Now we will suppose the machine to be in a quiescent state; the galvanic magnet, No. 1, being opposite the south pole of the artificial magnets the galvanic magnet, No. 3, will, of course, be opposite the south pole, No. 6, and the galvanic magnets, No. 2 and 4, will be opposite each other, between the poles just mentioned.

There being a corresponding number of copper plates, or conductors, placed below the artificial magnets around the shaft, but detached from it, as well as from each other, with wires leading from the galvanic magnets to these plates, and in contact with them, as before described, these wires will stand in the same position, in relation to the copper plates, that the galvanic magnets stand to the artificial magnets, but in contact with the plates.

Now in order to put the machine in motion, the galvanic magnet, No. 2, being charged by the galvanic current passing from the copper plate of the battery, along the conductors and wires, becomes a north pole, whilst, at the same time, the magnet, No. 4, is charged by the galvanic current passing from the zinc plate of the battery, and becomes a south pole; of course the south pole of the artificial magnet, No. 6, will attract the north pole of the galvanic magnet, No. 2, and will move it a quarter of a circle; the south pole of the galvanic magnet, No. 4 being at the same time attracted by the north pole, No. 5, causes the said magnet, No. 4, also, to perform a quarter of a circle: the momentum of the galvanic arms will carry them past the centres of the poles, No. 5 and 6, at which time the several wires from the galvanic magnets, will have changed their positions in relation to the copper plates, or conductors:—For instance, the north pole, No. 2, having now become a south pole, by reason of its wires being brought in contact with the conductors of the zinc plate, and No. 4 having, in like manner, become a north pole, its wire having changed its position from the zinc plate to the copper plate, the poles of the galvanic magnets are, of course, now repelled by the poles that before attracted them; and in this manner the operation is continued, producing a rotary motion in the shaft, which motion is conveyed to machinery, for the purpose of propelling the same.

The discovery here claimed, and desired to be secured by Letters Patent, consists in applying magnetic and electro-magnetic power, as a moving principle for machinery, in the manner above described, or in any other substantially the same in principle.

THOMAS DAVENPORT.

Remarks by the Editor.—The subject of the forgoing specification is one of great interest, and it has arrested a corresponding portion of public attention; we are likely soon, therefore, to have the question solved, whether this new power can be advantageously applied to the propelling of machinery as a substitute for the steam engine. Most of our readers, it is presumed, have seen Professor Silliman's notice of Mr. Davenport's machine, published in the Journal of Science, in April last, which contains much information respecting the attempts which had been made for the producing of motion by electro-magnetic apparatus. Since that period, advice has been received from Europe, showing that experiments upon this subject are in progress under the direction of some of the most distinguished Philosophers in various portions of that quarter of the globe.

We do not know by whom, or at what date, the first successful experiment of producing a direct rotary motion, by the electro-magnetic apparatus, upon a principle analogous to that upon which Mr. Davenport has proceeded, was performed. As early, however, as June, 1833, an article appeared in the London Mechanics' Magazine, proposing such an apparatus, and giving a figure of one which it was supposed, would answer the purpose; a supposition which was, undoubtedly, well founded. Not long after this, Mr. Saxton, we believe, produced a rotative machine by electro-magnetism, but we are not informed respecting its particular arrangements.

The history of the production of the machine patented by Mr. Davenport, is a history of the successful efforts of an individual, who to an indomitable perseverance, must have superadded, extraordinary natural abilities. His business is that of a blacksmith, and his advantages in point of education were not greater than usually falls to the lot of persons in country places, engaged in such pursuits. Accident brought to his notice, one of Professor Henry's electro-magnets, which he eagerly purchased, under a conviction that he could render it available as a motive power; this was in the year 1833, and in July 1834 he had so far succeeded as to produce a rotative machine, and this he effected in a country village, unaided by scientific knowledge, by books, or by the encouragement of men of superior attainments, or with kindred spirits. Whatever may be the final result of his labours, his merits are of a high order, and he has proved himself well worthy of the most splendid success. Should his machine finally accomplish that which he and many of his friends anticipate, its value will be incalculable, for although he may have been superceded in Europe, his claim as inventor will undoubtedly prove valid in his own country, and ambition need not carry him beyond it. We have twice seen his machine in operation, formerly in New York, and recently in Washington, where it was exhibited to the President, and the Heads of Departments. So far as the evidence of a model is to be taken, its performance is quite satisfactory; and Mr. Davenport is now occupied in constructing one which is intended to drive a Napier Press, requiring a two-horse power. This, should it succeed, will be a fair test of its value, and we confess that, although our expectations do not generally partake of the sanguine in such matters, not only our hopes, but we may say our confidence, has increased as we have become acquainted with the progress of the experiments which are being carried on.

We are well aware, that should it be eventually proved, that an available power may be obtained, which may be substituted for that of steam, its adoption would depend entirely upon its economy, with respect to which we cannot have satifactory data until a machine of several horses power shall have been produced; the probability, however, is, that the cost of operating the electro-magnetic apparatus, will be much below that of the steam engine.

Progress of Practical and Theoretical Mechanics and Chemistry.

On the use of Steam in the Economising of Fuel. By Andrew Fyfe, M. D., &c. Edin.

Dr. Fyfe caused steam to pass through a porcelain tube, stuffed sometimes with charcoal, sometimes with coke heated to redness in a furnace, collected the resulting gas, generally over a water trough, but sometimes over mercury. His inference from a variety of experiments is, that the gas contains hydrogen, oxygen, and carbon, and that the two last are in the state of carbonic oxide. Hence the combustion of the gas gives rise to the formation of carbonic acid and water.

When air was freely admitted to the incandescent material, at the same time that steam was driven through it, Dr. F. found that the water of the steam was, in part at least, consumed, and that the heat was thereby augmented. This was proved by the greater quantity of water evaporated in a given time. On an average, for each ounce of steam thrown into the furnace, there were four ounces additional evaporated, over and above that evaporated without the transmission of steam, provided the steam was thrown in cautiously. This increase of temperature, by the use of steam as a fuel was not effected at the expense of a greater quantity of fuel, for there was rather less fuel consumed when steam was transmitted through it than when omitted, while at the same time the quantity of water evaporated was increased. To arrive at correct results, it is necessary to throw the steam in cautiously.

It is thus proved that water, while passing in the state of steam through fuel, not only acts as a sort of blast, but, at the same time, itself undergoes combustion, by the formation and consequent consumption of inflammable gaseous products; and the increase of heat, Dr. F. thinks, will more than compensate for any extra expenditure for converting the water into vapour.

The author states that the only instance which he had found on record in which steam was passed through fuel with any definite object in view, is mentioned by Mr. Mushet in the 6th volume of Tilloch's Magazine. Had he consulted the early volumes of Silliman's Journal, and also the 25th vol. of that work, he would have found a succession of papers by Samuel Morey, in some of which the use of water as a fuel is distinctly alluded to, and in the last paper its application is proposed by that indefatigable experimenter to one or two practical purposes.

The foregoing account of Dr. Fyfe's experiments is abstracted from his papers in Jameson's (Edinburgh) Journal, No. 45. G.

Bunnett's Revolving Iron Safety Shutters.

There is scarcely any thing more desirable in populous towns and cities, than a ready and convenient method of securely closing the houses and shops of the inhabitants. The modes that have been devised for effecting this purpose are very various; but up to a comparatively recent period, most of the plans in common use were more or less inconvenient—many of them excessively clumsy, both as regarded the material and the manner of its application. The trouble of putting up every night, and taking down every morning, from ten to twenty heavy wooden shutters, together with the difficulty of finding a suitable place for stowing them away in the daytime, were among the most prominent evils entailed upon shopkeepers by the old-fashioned methods.

The cellar was very often the only eligible place of depository, from whence the shutters were drawn out across the foot-pavement of an evening, to the great and grievous annoyance of the pedestrians, with which the thoroughfares are at that time crowded.

Sundry remedies have been proposed for these evils, and of several improved shutters that have appeared, the most recent, and, in my humble opinion, the best, are Bunnett's Revolving Iron Safety Shutters, for which his Majesty's royal letters patent were obtained in June last. Although these shutters bear a slight resemblance to another invention for a similar purpose, they are essentially different in a most important particular.

Bunnett's shutters consist of a series of narrow plates or strips of iron, placed horizontally, and connected together by hinges of a simple construction, rivetted on to their under surface, in such a manner, that the iron plates overlap each other, and entirely conceal the hinges from external observation; considerable advantage arises from this circumstance, inasmuch as the hinges cannot be seen on the outside of a building; they are therefore not liable to be cut, nor can the knuckle-pins of the hinges be removed. The pieces of metal to form the hinges are struck out of plate iron, and afterwards bent up into the required form; each hinge being held to the strip forming the shutter by three rivets. The knuckles of each hinge being bent in opposite directions, allows the strips of iron to overlap each other; at the same time, the combined shutter is capable of bending over and winding round the roller, to which the first set of hinges is fastened. This arrangement entirely obviates the necessity for cutting away any portion of the substance of the shutter itself.

A front view of a window closed with one of Bunnett's patent shutters, has a light and elegant appearance, closely resembling a Venetian blind, the resemblance being greatly increased when they are painted (as they should be) green.

The following method is adopted for opening and closing these shutters. An iron roller with a toothed wheel at one end, is fixed in suitable bearings on each side at the top of the window; a worm, or endless screw on the top of a vertical shaft, takes into this toothed wheel, while the lower end of the shaft carries a small bevelled wheel, working into another similar wheel placed at right angles with it. The axle of the last bevelled wheel is squared off for receiving the handle, the turning round of which raises or lowers the shutter.

The compact form of these shutters requires no deviation from the present mode of building, but that of placing the lintel over the window a few inches higher than usual, to admit the shutter when rolled up.

The same principle may also be advantageously resorted to in fitting up valuable libraries; it is equally well adapted to the front of glass cases, &c. containing jewellery, or other valuables. As a temporary partition, for the division and protection of shops, the iron shutters will be found superior to every other contrivance.

The ends of the shutter slide up and down in two metal grooves, one on each side of the window-frame; there are two steady-pins in the window sill, which fit into corresponding holes in the lowest bar of the shutter; on the inside of this bar, one or more square holes are made for the reception of self-acting spring-bolts, which catch the shutter on its descent, and most effectually prevent its being raised by any force either internal or external, until they are withdrawn. These bolts may be replaced by locks, where the shutters are not to be opened but in the presence of the owner.

Among many other advantages offered by this ingenious invention, the following are some of the most striking:—Perfect security in all situations against both fire and thieves; the simplicity of construction, and the nature of the materials employed, which ensure their continuance in good and efficient working order; the short space of time required to open or close them (a few seconds,) and the ease with which it is accomplished, enabling even females or children so manage them without any liability to accident. They are equally adapted to the house of business or private residence, the absence of inside shutters allowing the windows to be finished in any way that taste or necessity may dictate.

Another most important advantage would be gained by fixing these shutters to fire-places generally, so as to fall in front of the stove when fires are left, or extinguished for the night, and thereby preventing those accidents which frequently arise for want of such a provision. In the event of the chimney taking fire, the shutter would enclose the falling particles of ignited soot, and by stopping the draft, extinguish the fire, thereby removing the cause of danger.

Although the first year of Mr. Bunnett's patent is not yet expired, his shutters have already come into extensive employment. WM. BADDELEY.

May 25, 1837.

Mech. Mag.

The Pine Apple Plant and its Fibre.

This plant, which has hitherto been valued solely as ministering to the luxuries of the table, has lately had a new interest attached to it from the discovery of a fibre contained in its leaves, possessing such valuable properties, that it will, in all probability, soon form a new and important article of commerce.

This fibre is found, on comparison, far to surpass in delicacy of texture, those materials which now form the basis of our woven manufactures: of these the principal are silk, wool, cotton, and flax. Silk is a continuous fibre, often extending, without interruption, to the length of 1000 feet; viewed under the microscope, it is found to be perfectly cylindrical, beautifully smooth, glossy, and transparent. The best kind of prepared silk varies from $\frac{1}{1700}$ to $\frac{1}{2000}$th part of an inch in diameter. Wool, on the other hand, is a rough cylindrical fibre, which appears as if plated with irregular scales, the edges of which overlap each other; though invisible to the naked eye, its roughness may be easily detected, by drawing a fibre between the fingers, in a direction from its end to its root. It varies greatly in both length and size, the diameter of very fine wool being from $\frac{1}{700}$th to

$\frac{1}{1200}$th part of an inch. Cotton, being the down of a seed-pod, has peculiar characters, a thin continuous tube, presenting the appearance of a flattened and twisted cylinder, arising, probably, from the compression which the fibre undergoes before the opening of the pod: its diameter is about the $\frac{1}{1000}$th part of an inch. Flax, in many respects, differs from the foregoing materials; a filament of silk, wool, or cotton, admits of no division, while, on the other hand, each filament of flax is a fasciculus or bundle of fine fibres, connected by a thin membrane, and the natural gluten of the plant. These fibres may be termed ultimate, as they admit of no further subdivision: they are cylindrical tubes, disposed parallel to each other, and having the appearance of innumerable joints; their diameter varies, in different flaxes, from $\frac{1}{700}$th to $\frac{1}{2500}$th part of an inch.

The fibres of the pine-apple plant are also disposed in fasciculi, each apparent fibre being an assemblage of fibres adhering together, of such exceeding delicacy, as only to measure from $\frac{1}{5000}$th to $\frac{1}{7000}$th part of an inch in diameter; viewed under the microscope, they bear considerable resemblance to silk, from their glossy, even, and smooth texture. They appear altogether destitute of joints, or other irregularities, and are remarkably transparent, particularly when viewed in water: they are very elastic, of great strength, and readily receive the most delicate dyes. This last fact appears singular, when we bear in mind the resistance, if we may be allowed the expression, which flax offers to dyes. With much trouble, and by long processes, flax will receive a few dark dingy colours: all light and brilliant ones it wholly resists, they do not enter the fibre, but merely dry upon it externally, and afterwards easily peel, or rub off,—in short, it may be said to be *painted*, and not *dyed*.

The preparation of the pine-fibre is exceedingly simple. If a leaf of this plant be examined, it will be found to consist of an assemblage of fibres running parallel from one extremity of the leaf to the other, embedded in the soft pabulum. All the process necessary is to pass the leaf under a "tilt hammer," the rapid action of which, in a few seconds, completely crushes it, without in the slightest degree injuring the fibre, which remains in a large skein, and then requires to be rinsed out in soft water, to cleanse it from its impurities, and be afterwards dried in the shade. So simple and so rapid is the process, that a leaf, in a quarter of an hour after being cut from the plant, may be in a state fit for the purposes of the manufacturer, as a glossy, white fibre, with its strength unimpaired by any process of maceration, which, by inducing partial putrefaction, not only materially injures the strength of flax, but also renders it of a dingy colour.

The pine-plant abounds both in our East and West India possessions, and may be easily propagated from the crown; the offsets from round the base of the fruit, which often amount to upwards of twenty in number; and from the young plants which spring from the parent stem; its cultivation requires but little care or expense, and the plant is of such hardy growth, as to be almost independent of those casualties of weather, which often prove so detrimental to more delicate crops—it is one of those plants which Nature has scattered so profusely through tropical regions, whose leaves are thick and fleshy, to contain a large supply of nourishment, and covered by a thick, glazed cuticle, which admits of so little evaporation, that many of the tribe will thrive upon a barren rock, where no other plant could live. From the arge portion of oxalic acid which the leaves also contain, no animal will ouch them, and they are, therefore, exempt from the tresspasses of cattle, &c., indeed, no greater proof of the hardiness of the plant can be given, than the fact, that in many places where lands have been under tillage, and have afterwards been abandoned, and allowed to return to a state of nature, the pine-apple plants form the only trace of former cultivation; every other cultivated plant has died away before the encroachments of the surrounding wood, while they alone have remained increasing from year to year, and have spread into large beds.

In adverting to the present state of our West Indian colonies, we cannot but think that the discovery of this fibre will prove to them a most valuable acquisition. The small amount of labour and of capital requisite for the cultivation of the plant, its hardy growth, its abundant produce, the facility with which the fibre may be prepared from its leaves, the trifling cost at which it can be brought to market, and the value attached to it, as an article of commerce, by those manufacturers to whom it has been submitted, seem fully to justify the opinion that it is calculated to open a new source ot wealth to the colonies, and become a staple and valuable article of British manufacture. Mag. Pop. Sci.

On the Preparation of Sulphuret of Carbon.

M. Mulder directs, that in an iron bottle in which mercury is imported, besides the hole which is already there, another should be bored near it. Into the first of these openings a copper tube $\frac{4}{10}$ inch diameter, bent twice at right angles is to be screwed, and into the second a straight tube, also of copper, is to be introduced. Then the bottle is to be filled with pieces of charcoal, recently heated to redness, of such a size that they can easily pass down the tube. After having firmly screwed in the straight and curved tubes, place the bottle in a furnace and heat it, after having closed the opening of the furnace with a stone cut in halves to prevent inconvenience to the operator from the ascending heat.

Adapt to the curved tube a Woulff's bottle half filled with water and surrounded with a freezing mixture, and when the iron bottle is sufficiently heated, introduce by the straight tube fragments of sulphur and immediately close the mouth of the tube with a plug; the sulphur fuses, and falling upon, penetrates the pieces of charcoal, and when the sulphur is gradually added, but little gas is evolved and abundance of sulphuret of carbon obtained.—*Journal de Pharmacie*, Jan. 1837. Lon & Ed. Philos. Mag.

Researches made on the Bouquet of Wines. By Messrs. LIEBIG AND PELOUZE.

It has been long suspected that wine contained a peculiar principle which is the cause of the agreeable odour generally known as the bouquet of wines, which principle seems hitherto to have eluded all the attempts which have been made to detect it. "We have now," however, remarks the authors of this extract, "the honour of presenting to the Academy certain experiments which we have made on an essential oil, which was sent to us by M. Deleschamps of Paris, and which, from all its properties, appears to be the principle so long sought after. This substance has an exact resemblance to the essences, and was given to us as such; its odour is completely that of old wine, with the exception of its intensity. Its chemical properties remove it from the class of essential oils, and regarding its constitution it sheds a new light on organic chemistry, in supplying the first well established example of the existence of a true ether, which is produced in the act of fermentation, and without the aid of the chemist. This ether is composed of an atom of sulphuric ether, and an atom of a new acid which we propose to designate *œnanthic* acid, and which is formed of $C^{14} H^{26} O^{2}$. The condensation of its vapour is that of formic and acetic ethers. The oil presents itself under the form of a greasy oil which crystallizes at 56° Fahr. In its free state it contains one atom of water, which it loses by distillation. By combining it with sulphuric ether, we can easily reproduce *œnanthic ether*. We have deemed it necessary, for the purpose of establishing the composition of one of the most remarkable of these acids, viz. the *melitic acid*, to make some analytic investigations concerning its combination with oxide of silver. We believe that this acid may be regarded as a hydrous acid, and our experiments on this point confirm the views of M. Dulong respecting oxalic acid. The melitate of silver dried in vacuo, by the side of sulphuric acid, contains some hydrogen, which it loses only at the temperature of 356° Fahr. under the form of water, and with change of colour. There is not one other salt of silver which exhibits the same result; all the others are anhydrous. In this particular instance, the formation of water appears to be the consequence of the reduction of the oxide, and not a simple volatilization of water which had pre-existed in the salt. According to all known analyses, the melitic acid contain three atoms of oxygen. We have reason to think it contains four, and besides two atoms of hydrogen; and that this hydrogen enters into the constitution of all the melitates except into that of silver heated to 356°, so that this last represents a combination of metallic silver, with the radicle of hydrous acid." Ed. New Philos. Jour.

Hot Air Blast.

The benefits which this new process appeared to promise, and the sanguine expectations of economy and profit that have been entertained, do not appear to be fully realized. Still it is not without its advantages, and with respect to the use of anthracite coal in the reduction of iron ore, the application of the hot blast has evidently afforded, under the management of Mr. Crane, in Wales, the most encouraging indications of a success, which in its application to our own mines of coal and iron may prove of incaluable importance..

The following extracts from a communication of W. Wood to the London Mining Journal, point out what he deems to be the causes of failure in the use of hot air in the smelting of iron, and also the incidental benefits which it already produces. It is much to be desired that experimenters and iron masters may not be too soon discouraged by the unfavourable nature of these statements, for it is very possible that the means may be hit upon of obviating the causes which appear to occasion a deterioration of the quality of the iron, and still secure to the hot air process all the facilities and advantages to which both science and fact most evidently give it a claim. G.

The use of heated air in smelting, has greatly facilitated the production of grey iron, and has, in many instances, occasioned a material increase in the make; but that it has been productive of the great advantages which were at first supposed to accrue from its use, has been by no means established. I do not mean to dispute, that at some concerns, a reduction in the quantity of fuel made use of may have been effected, but that this has been the general result, I believe I may safely say, is by no means the case; on the contrary, in the majority of the works where it has been made use of, the saving effected is a mere bagatelle—the aggregate quantity consumed in smelting, and heating the air, being very nearly equal to that which had been previously used in smelting with cold-blast. From all I can collect from different parties who have adopted the system, the only advantage they derive from it is, that the furnaces work more uniformly, not being liable to the fluctuations which are experienced when cold-blast is made use of. This is undoubtedly an advantage of considerable importance, and would be sufficient to recommend its universal adoption, were there no disadvantages connected with its use, which more than balance all the benefits derived from it, and must permanently operate against its general introduction. What I allude to is, the reduction of the actual strength of the iron produced by this process. That this is a fact, is now established beyond a doubt, by numerous experiments by different parties. I sometime ago instituted a series of experiments myself, on the relative strength of iron produced by the old and new process. The bars I made use of were one and one-eighth square, and supported at each end on supports two feet distant from each other; from the centre I suspended a scale board, on which weights were placed, and increased seven pounds at a time, until they broke. The bars were obtained from a neighbouring iron works, and were produced from the same materials, one set from cold-blast, and the other from hot, and were to all appearance of precisely the same quality, so much so that there was not the least apparent difference in the quality of each on inspecting the several fractures. The averaged result was, that the cold-blast iron broke under a weight of 2009 lbs., and the hot-blast iron under a weight of 1568 lbs. This shows a deficiency of more than twenty per cent. in the strength of the hot-blast. The deflection under equal weights was rather in favour of the hot-blast iron, proving it to be less flexible until it arrived at the breaking point. This is a most serious objection to such iron, and operates against its safety, when applied to purposes requiring great strength.

I have never seen any satisfactory reason assigned for the deficiency which is evident in its strength; it, however, in my opinion, admits of a solution on chemical principles. Sir Humphrey Davy was the first person who established the probability of the different earths being oxydes of metals, reducible to the metallic state in the same manner as the ores of other metals; and Berzelius succeeded in forming an alloy of silicium and iron, thereby establishing the fact of their affinity for each other. That the earths, in combination with the ironstone and other materials made use of in the blast furnace, are in small proportions reduced along with the oxyde of iron, there cannot be a reasonable doubt, and is in a measure proved by the analysis of pig-iron, which is invariably found in combination with alumine, lime, and silex, in various proportions. These there is every reason to believe, are in combination with the iron in a metallic state, thereby forming an alloy, whose force of cohesion is much less than that of pure iron, and in proportion to the quantities of these earths revived will its strength be affected. I am the more confirmed in this opinion by the result of an analysis which I made some years ago of iron produced from ironstone and from refinery cinder, the former producing much stronger iron than the latter. This analysis showed that there were three per cent. of earthy matter in the stronger iron or that produced from ironstone, and five per cent. in the weaker or that produced from refinery cinder. Both samples were smelted at the same iron works, with the same coal and limestone, and under precisely similar circumstances, therefore the fair inference to be drawn from the analysis is, that the earthy matter, in a metallic state, was the primary cause of the deficiency in the strength. The predominant earth in each case was silex, from which we may infer that this earth has a weaker affinity for oxygen than alumine and lime, and is consequently more readily reduced to the metallic state than either of the others. That the affinity of silex for iron is strong, is proved by the circumstance that the refining processes made use of to produce malleable iron, do not entirely remove it, for it is found in the finished bar, in quantities bearing a proportion to that found in pig-iron, although of course in smaller quantity.

By the old process, the temperature of the furnace is much reduced by the introduction of the immense quantity of cold-air required to carry on combustion, and which must necessarily abstract a great quantity of caloric to elevate it to the temperature of the mass of matter in the furnace, and that, in the vicinity of the twyers, where intense heat is most required, for the purpose of producing perfect fusion of the earthy part of the materials, to effect a thorough separation of the metal from them. By the new process of heating the air previous to its introduction, its refrigerating effect is, in a great measure obviated, the temperature is kept up, and preserved in that state best calculated to increase the affinities of the oxygen in the ore, and of the iron when reduced to a metallic state, for the carbonaceous matter with which it is in contact, thereby facilitating the combination of a larger portion of carbon with the iron than is effected with cold-blast. If the furnace, by this process, is kept in a state better adapted to reduce the oxyde of iron, it also stands in the same position as regards the earths, and there cannot be a reasonable doubt as to the fact of their being revived in larger proportion than under the cold-blast system; and to this cause, I am persuaded, is to be attributed the deterioration in the quality as far as its strength is concerned. To satisfy myself on this point as far as possible, I analysed the iron, the experiment on the strength of which I have before noted, the result of which goes far to confirm this opinion, and is as follows:

From 100 parts of cold-blast iron I obtained

Silex	2.33	or Silicium	1.165
Alumine	0.78	or Aluminum	545
Lime	0.26	or Calcium	186
	3.37		1.896

And from 100 parts of hot-blast iron

Silex	4.27	or Silicium	2.135
Alumine	1.10	or Aluminum	0.767
Lime	0.41	or Calcium	0.399
	5.78		3.301

These analyses correspond very nearly with those before mentioned, and, in my opinion, leave little doubt as to the inferior quality of the iron produced with hot-blast being attributable to the same cause as that which operates against that produced from refinery cinder, which has hitherto baffled every attempt to improve its quality. The cinder here alluded to, is that produced in the first process, made use of in converting cast into malleable iron, which is technically termed, refining. That produced in the subsequent processes, makes iron of a much better quality, and in many cases a judicious mixture of it, improves the quality of that produced from ironstone.

I have not had an opportunity of fairly trying the effect of hot-blast on the strength of malleable iron, on a sufficiently extensive scale to speak decidedly of it from my own knowledge, but from information I have received, from a person of good practical knowledge, and recently employed as a superintendant over a part of the manufacturing department of an extensive iron works, where the principle is applied to one furnace, I have every reason to believe that the effect is equally injurious to bars as to cast-iron; at any rate, as far as regards the iron produced at this particular works, and where that produced with cold blast is generally good. What this person states is, that the iron produced there is excessively weak and coldshort, so much so that a very small proportion of it can be made use of, mixed with the other iron. This is precisely the character of refinery cinder iron, to which it is in most respects analogous.

Circumstances may probably operate in some situations to modify the character of it, but whether sufficiently to make it desirable to adopt it, is what I am not at present prepared to speak to, experience alone can decide this point.

I am, Sir, your obedient servant,

W. Wood.

Abersychan Iron Works, Pontypool,
Aug. 5, 1837.

Min. Jour.

Iron Trade in Scotland.

It appears from a statement of Bald and Geddes, mining engineers, that it is about seventy years since iron-works were erected in Scotland, at Carron, in Stirlingshire, and for twenty years afterwards, the annual produce of pig-iron there, was under 8000 tons. In forty years after Carron Works were in operation, or about 1805, seven additional iron-works appear to have been built, and about this period the yearly make of iron in Scotland might be about 24,000 tons. Only one more iron-work appears to have been built in the succeeding twenty years; and, in 1825, we find the annual produce of iron increased to 29,200 tons, the number of furnaces then built being twenty-five, of which only seventeen were in blast.

At the beginning of the current year, there were thirty-seven furnaces at work, producing at the rate of 99,500 tons of pig-iron annually, being an increase of twelve furnaces in twelve years, and an additional yearly make of iron of 69,000 tons, compared with 1825. Besides this increased number of furnaces in operation in 1837, there were five additional furnaces in progress of building, and four more contemplated, which, when completed, will produce 27,000 tons more iron yearly; the annual make of iron in Scotland, would thus be 126,500 tons, or about *one-sixth* part of the gross estimated make of iron in Great Britain yearly. In 1825, Scotland only produced about *one-twentieth* part of the make of iron in the United Kingdom.

This extraordinary increase in the iron trade of our country is principally found to have taken place in the county of Lanark, and the apparent physical causes which have contributed to the recent rapid extension of this trade in the Lower Ward of that county, may be considered as threefold.

1st. The moderate depth to, and thickness of, the seams of coal, which enables them to be readily and cheaply produced.

2nd. The abundant supply of what is locally called blackband ironstone, which, being of a bituminous nature, is easily calcined; and 3dly, the late discovery, and now general application at the iron-works of that county, of Neilson's hot blast, which has produced great economy in the quantity of coal requisite to produce good pig-iron; this improved mode of blast is also applied at all the other iron-works of Scotland. This blackband ironstone is as yet chiefly peculiar to the parishes of Old and New Monkland, where it is found from a few inches to about two feet thick: and it is remarkable, that though about forty years have elapsed since its discovery by Mr. David Mushet (who, in the opinion of a late eminent iron-master, deserved a crown of gold for the discovery,) its general application in iron-making, is but of recent date. The importance attached to this ironstone by the iron-masters in the west, was distinctly marked in the fall of last year, by their uniting to pay a yearly rent of £12,000 for liberty to work it in part of the estate of Airdrie, extending to about 300 acres, where it is of good quality, and possesses peculiar facilities for being worked; and this circumstance has induced many proprietors in the adjoining parishes and counties to make trials to ascertain if this valuable ironstone be extensively deposited in the coal-fields there; some of these trials have proved successful, while others have entirely failed.

The editor of the *Merthyr Guardian* makes the following remarks:—

"We understand the Blackband Ironstone therein mentioned, and on which the extent and prosperity of the iron trade in Scotland chiefly depend, has been found in North Staffordshire, and lately (to the extent of 500 acres) in the estates of Lord Lichfield, in South Staffordshire. Something analogous, as it respects its conformability and connexion with the coal measures, has also been found at Bellingsley, in Shropshire, above two feet in the cross, and containing forty per cent. of iron, with a calcareous, not a carboniferous base, and there appears to us no good reason why the same, or a similar formation, may not be found (upon proper search being made) to pervade the South Wales Coal Basin; at any rate, we consider that we discharge an important duty in thus drawing the attention of the iron masters of this district to the subject."

The following letter from Mr. Mushet has since appeared in the *Scotsman*, "and refers," says the Editor of that Journal, "to the interesting communication of Messrs. Bald and Geddes, on the iron trade of Scotland and the Blackband ironstone." The discoveries and experiments detailed in Mr. Mushet's letter, appear to us peculiarly valuable, and well deserving the attention of the iron masters of South Wales:—

Sir,—I have read, with a good deal of interest, in your excellent and highly talented newspaper of the first of July, a statement of Messrs. Bald and Geddes, on the past, present, and probable future state of the iron trade in Scotland; the prospective advantages of which seem to rest in a great measure upon the general extension of the Blackband ironstone in the coal districts, and of obtaining a plentiful supply, for smelting, of that easy fusible and highly valuable carboniferous (not bituminous) ironstone.

I may be excused for taking an interest in the development of the question, as I happened to be the person referred to in the above paper who first made the discovery, and dared to apply it to the purpose of smelting, about thirty-four years ago, amidst the sneers and pity of the iron veterans of those days, at my folly in attempting to make iron from wild coal—for this was the name by which the Blackband was known, previously to its application to iron-making by myself.

As this ironstone has now become an object of considerable national importance, perhaps you will allow me room to allude more particularly to the circumstances of its discovery. Thirty-six years ago, this summer, in attempting to cross the river Calder on foot, in the Old Monkland Parishes, I obtained nearly a dry footing upon what appeared to be a continuous black pavement, from the upper edge of which I struck off a piece of substance resembling coal, which, upon a closer examination, was found interlaminated with yolks of brownish matter, about three-eights of an inch in thickness. These, when separated from the carboniferous schist, proved to be good ironstone, but so small in quantity, that the average of the mass did not exceed 13 per cent. Next day I returned to the black pavement, better prepared to ascertain its extent and thickness. On removing a portion of the entire bed, I found it to be fourteen inches in thickness, viz., three and a half of coaly schist with the before-mentioned yolks, and ten and a half of more compact and solid matter, but still far removed from the usual appearance of ironstone or iron ore—its specific gravity being about 2.15. It was soon ascertained that this portion, being the richest, contained thirty-three per cent.—the average of the whole bed thirty per cent., in the raw state; but being entirely carboniferous, it was found to lose fifty per cent. in roasting, which was, and no doubt still is, performed without the addition of any fuel, thereby forming an ore perfectly prepared for the furnace, containing from sixty to seventy per cent. of iron. In a short time from making this discovery, I had ascertained the existence of the Blackband in 2000 acres of land, including the Airdrie estate (for right of working in which, to a limited extent, £12,000 a year is now about to be given,) and determined it to be a regular member of the coal formation.

Before venturing it in the blast-furnace, my experiments were numerous and are still extant; and even at this distance of time ("*the crown of gold*" being still in abeyance) may be referred to with satisfaction, as descriptive of that diversity of quality, form, and structure, so peculiar to this ore in roasting; not the least singular of which is, the occasional production of thin sheets of malleable iron interposed between lamina of the carboniferous schist.

I may further remark, in reference to Messrs. Bald and Geddes's paper, above alluded to, that although the use of the Blackband ironstone at the blast furnaces in Scotland may not have been so general as might have been expected; yet I believe its application has been constant at the Calder furnaces, from the period of its first introduction, and also at the Clyde furnaces for ten or twelve years past.

The removal of prejudice, and the consequent march of intellect, are, however, always slow, as I have recently experienced upon this subject; for, with some knowledge of the nature and quality of the bar iron likely to result from the Blackband (particularly if smelted with hot-blast,) and with a view to meet or remove the real or apprehended difficulties, I wrote to a party concerned in the welfare of the manufacture four months ago, but so *slow is the process of inter-communication in this country*, that the reply to my letter has not yet reached me.—I am, &c.

Coleford, Gloucestershire, July 12. DAVID MUSHET.

Ibid.

Progress of Physical Science.

SIR J. F. HERSCHEL *on the theory of Volcanic Phenomena.*

Extracts from a letter from Sir John F. W. Herschel to C. Lyell, Esq., dated Fredhausen, Cape of Good Hope, 20th February, 1836.

The author commences by inquiring, whether it had ever occurred to Mr. Lyell to speculate on the probable effect of the transfer of pressure from one part to another of the earth's surface by the degradation of existing, and the formation of new continents, on the fluid or semifluid matter beneath the outer crust? Supposing the whole to float on a sea of lava, the effect would merely be an almost infinitely minute flexure of the strata; but supposing the layer next below the crust to be partly solid and partly fluid, and composed of a mixture of fixed rock, liquid lava and other masses in various degrees of viscidity and mobility; great inequalities may subsist in the distribution of pressure, and the consequences may be local disruptions of the crust where weakest, and escape to the surface of lava, &c.

Referring to the phenomena of volcanos, Sir J. Herschel observes, that it has always been his greatest difficulty in geology to find a *primum mobile* for the volcano, taken as a general and not as a local phenomenon; and referring to the different theories given on the subject, which he considers insufficient, wanting in explicitness and as not going high enough in the inquiry or up to its true beginning, and also as giving in some respects a wrong notion of the process itself;—inquires, how came the gases which are evolved to be condensed?—why did they submit to be urged into liquefaction?—if they were not originally elastic, but have become so by subterranean heat—whence came the heat, and why did it come?—how came the pressure to be removed, or what caused the crack?

It seems clear that if the gases or aqueous vapour were once free at so high a degree of elasticity as is presumed, there exists no adequate cause for their confinement. We are forced therefore to admit that the elastic force has been superadded to them during their sojourn below by an accession of temperature.

Assuming a high central temperature, which many geologists admit, and with which all are familiar; the author agrees with Mr. Lyell's observations, that the ordinary repose of the surface argues a wonderful inertness in the interior, where in fact he conceives that everything is motionless; debarred therefore from the invasion of a circulating current or casual injection of intensely hot liquid matter from below, he conceives that the phenomena may be explained as follows:

Granting an equilibrium of temperature and pressure within the globe, the isothermal strata near the centre will be spherical; but where they approach the surface they will by degrees conform themselves to the configuration of the solid portion, that is, to the bottom of the sea and the surface of continents. If we suppose therefore a state of equilibrium, and that under the concave bottom of any great ocean the lines of equal temperature be parallel to its concavity; when this comes to be filled up by the deposition of matter brought down by rivers, &c., the formerly concave bottom may become horizontal or even convex, and the equilibrium of temperature will immediately be disturbed; because the form of a stratum of temperature depends essentially on the bounding surface of the solid above it, that form being one of the arbitrary functions which enter into its partial differential equation. The temperature, therefore, will immediately begin to migrate from below upwards, and the isothermal strata will gradually change their forms from the concave to the horizontal or convex form. The former bottom of the ocean will then (after the lapse of ages, and when a fresh state of equilibrium is attained) acquire a temperature corresponding to its then actual depth; while a point as deep below it, as itself is below the surface, will have acquired a much higher temperature, and may become actually melted, and this without any bodily transfer of matter in a liquid state from below. But if the temperature of this supposed deep stratum be already at the melting point, then will this rise to the former bottom of the ocean and the strata become melted, *water included*, with which, from the circumstances of the case, they must be saturated.

If the process of deposition go on, until by accumulation of pressure on the bottom or sloping sides, some support gives way,—a piece of the solid crust breaks down and is plunged into the liquid below, and a crack takes place, extending upwards. Into this the liquid will rise by simple hydrostatic pressure. But as it gains height it is less pressed; and if it attain such a height that the ignited water can become steam, the joint specific gravity of the column is suddenly diminished and up comes a jet of mixed steam and lava; till so much has escaped that the deposited matter takes a fresh bearing, when the evacuation ceases and the crack becomes sealed up.

By taking this view of the process of heating from below, we have a strictly theoretical explanation of the effects of heat on newly deposited strata; and this, simply because the fact of a new stratum having been deposited, the conditions of the equilibrium of temperature become altered, and they draw the heat to them, or rather retain it in them in its transit outwards; the supply from the centre being supposed inexhaustible, and its temperature of course invariable.

As the greatest transfer of material to the bottom of the ocean is produced on the coast line by the action of the sea, while the quantity carried down by rivers from the surface of continents is comparatively trifling; hence therefore the greatest local accumulation of pressure is in the central area of deep seas, but the greatest local relief takes place along the abraded coast lines: here, therefore, according to this view should occur the chief volcanic vents.

In this view the effects of the removal of matter from above to below the sea, are, 1st. It produces a mechanical subversion of the equilibrium of pressure. 2d. It also, and by a different process, produces a subversion of the equilibrium of temperature. The last is the most important. It must be an *exceedingly slow process*, and will depend, 1st. On the depth of matter deposited; 2d. On the quantity of water retained by it under the great pressure; 3d. On the tenacity of the incumbent mass—whether the influx of caloric from below, which *must take place*, acting on that water, shall either heave up the whole mass as a continent; or shall *crack* it and escape as a submarine volcano; or shall be suppressed until the main weight of the continually accumulating mass breaks its lateral supports at or near the coast lines, and opens there a chain of volcanos.

Thus the circuit is kept up—the *primum mobile* is the degrading power of the sea and rains (both originating in the sun's action) above, and the inexhaustible supply of heat from the enormous resources below, always escaping at the surface, unless when repressed by an addition of fresh clothing at any particular part. In this view of the subject the tendency is outwards. Every continent deposited has a propensity to rise again, and the destructive principle is continually counterbalanced by a reorganizing principle from beneath. Nay, it may go further; there may be such a tendency in the globe to swell into froth at its surface, as may maintain its dimensions in spite of its expense of heat, and thus preserve the uniformity of its rotation on its axis.*

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* On the subject of the views here enunciated by Sir John Herschel, see a notice of a paper by Mr. Babbage, Lond. and Edinb. Phil. Mag. vol. v, p. 213. Mr. Babbage has since published the substance of his paper in his work entitled "The Ninth Bridgewater Treatise," together with the extracts from Sir J. Herschel's letters, abstracted

An Extract of a letter from Sir John F. W. Herschel to R. I. Murchison, Esq., in explanation of the former, to C. Lyell, Esq., dated Fredhausen, 15th November, 1836.

In this letter the author recapitulates the views given in the foregoing abstract, stating that his views are not so much a theory as a pursuing into its consequences, according to admitted laws, of the hypothesis of a high central temperature; and his object to get a geological *primum mobile* in the natue of a *vera causa*, and to trace its working in a distinct and intelligible manner, so that in future, instead of saying as heretofore, "*Let heat* from below *invade* newly deposited strata, then they will expand, melt," &c., we shall commence a step higher and say, "*Let strata be deposited*, then, as a necessary consequence, and according to known regular and calculable laws, heat will gradually invade them from below and around; and according to its due degree of tensity at any assigned time will expand or melt them as the case may be," &c. The phenomena of earthquakes, volcanic explosions, &c., may arise, but if all goes on in quiet, the only consequences will be the obliteration of organic remains and lines of stratification, and the formation of new combinations of a chemical nature, &c.; in a word, the production of *metamorphic* or stratified primary rocks.

In the formation of these therefore there is nothing casual; all strata once buried deep enough, and due time allowed, must assume that state. None can escape; all records of former worlds must ultimately perish.

Lon. & Ed. Phil. Mag.

Agreement of Geology with Revelation.

Charles Babbage, Esq., Lucasian Professor of Mathematics of the University of Cambridge, has recently published a work, which is announced as a Ninth Bridgewater Treatise, and which will doubtless meet with a reception correspondent with the high reputation of the author. It contains an article on the supposed discrepancy between the facts of modern Geology and the account of the creation in the First chapter of Genesis. This subject has excited abundant discussion both in religious and philosophical Journals, and in essays, and treatises, especially devoted to it. It has been carefully and critically examined, as it well deserves to be, and the result of the investigation is evidently tending to the general conviction that there is no actual discord between the truths of scripture as contained in the Mosaic Revelation, and the indubitable records of Creation which are continally brought to light by geological researches. The evidence of the senses, confirmed by years of experience, in all parts of the world, no reasonable man will presume to dispute. The question then turns upon the right conclusions with respect to the age of the world; to which these archives of nature necessarily lead, and also upon the right interpretation of the sacred text. On these points the views of Prof. Babbage will be read with no little interest. We take the extract from a late number of the Edinburgh New Philosophical Journal. G.

Those, who, without the knowledge which enables them to form an opinion on the subject, feel any latent wish that this evidence (the records of

above. He observes, in reference to the similarity of Sir J. Herschel's views to those he had himself previously started, "I feel, that the almost perfect coincidence of his views with my own, gives additional support to the explanations I have offered; whilst the reader will perceive, from the different light in which my friend has viewed the subject, that we were both independently led to the same inferences by different courses of inquiry."—Edit.]

nature) should be overthrown, would do well to remember that geology also furnishes strong evidence in favour of the much more direct statement of Moses, as to the recent creation of Man. And although we must ever feel a certain degree of caution in admitting negative evidence as conclusive; yet, in the present instance, the multitude of fossil bones which have been discovered, and which, when examined by persons *duly qualified* for the task, have been uniformly pronounced to be those of various tribes of animals, and not those of the human race, undoubtedly affords strong corroborative evidence in confirmation of the Mosaic account.

In truth, the mass of evidence which combines to prove the great antiquity of the Earth, is so irresistible, and so unshaken by any opposing facts, that none but those who are alike incapable of observing the facts and of appreciating the reasoning, can for a moment conceive the present state of its surface to have been the result of only six thousand years of existence.

What, then, have those accomplished who have restricted the Mosaic account of creation to that diminutive period, which is, as it were, but a span in the duration of the Earth's existence, and who have imprudently rejected the testimony of the senses, when opposed to their philological criticisms? Undoubtedly, if they have succeeded in convincing either themselves or others, that one side of the question must be given up as untenable, those who are so convinced are bound to reject that which rests on testimony, not that which is supported by still existing facts. The very argument which Protestants have opposed to the doctrine of transubstantiation would, *if their view of the case were correct*, be equally irresistible against the book of Genesis.

But let us consider what would be the conclusion of every reasonable being in a parallel case; let us imagine a manuscript written three thousand years ago, and professing to be a revelation from the Deity, in which it was stated, that the colour of the paper of the very book now in the reader's hand is *black*, and that the colour of the ink in the characters which he is now reading is *white*,—with that reasonable doubt of his own individual faculties, which would become the inquirer into the truth of a statement said to be derived from so high an origin, he would ask of all those around him, whether, to their senses, the paper appeared to be *black*, and the ink to be *white*. If he found the senses of other individuals agree with his own, then he would undoubtedly pronounce the alleged revelation a forgery, and those who propounded it to be either deceived or deceivers. He would rightly impute the attempted deceit to moral turpitude, to the gross ignorance, or to the interested motives of the supporters of it; and he certainly would not commit the impiety of supposing the Deity to have wrought a miraculous change upon the senses of our whole species, and to demand their belief in a fact directly opposed to those senses; thus throwing doubt upon every conclusion of reason which related to external objects, and, amongst others, upon the very evidence by which the authenticity of that questionable manuscript was itself supported, and even of its very existence when before their eyes.

Thus, then, had those who attempt to show that the account of the creation in the book of Genesis is contradicted by the discoveries of modern science, succeeded, they would have destroyed the testimony of Moses, they would have uncanonized one portion of Scripture, and, by implication, have thrown doubt on the remainder. But minds which thus failed to trace out the necessary consequences of their own argument, were not likely to have laid very secure foundations for the basis on which it rested; and I shall presently prove that the contradiction they have imagined can have no real existence, that, whilst the testimony of Moses remains unimpeached, we may also be permitted to confide in the testimony of our senses.

Before entering on the main argument it may be remarked, that the plainest and most natural view of the language employed by the sacred historian of the Earth is, that his expressions ought to be received by us in the sense they were understood by the people to whom he addressed himself. If, when speaking of the creation, instead of using the terms light and water, he had spoken of the former as a wave, and of the latter as the union of two invisible airs, he would assuredly have been perfectly unintelligible to his countrymen. At the distance of above three thousand years his writings would just have begun to be comprehended, and possibly three thousand years hence, those views may be as inapplicable to the then existing state of human knowledge, as they would have been when the first chapter of Genesis was written.

Those, however, who attempt to disprove the facts presented by observation, by placing them in opposition to revelation, have mistaken the very groundwork of the question. The revelation of Moses itself rests, and must necessarily rest, on *testimony.* Moses, the author of the oldest of the sacred books, lived about 1500 years before the Christian era, or about 3300 years ago. The oldest manuscripts of the Pentateuch at present known, appear to have been written about 900 years ago. These were copied from others of older date, and those again might probably, if their history were known, be traced up through a few transcripts to the original author; but no part of this is revelation; it is testimony. Although the matter which the book contains was revealed to Moses, the fact, that what we now receive as revelation is the same with that originally communicated, is entirely dependent on testimony. Admitting, however, the full weight of that evidence, corrobated as it is by the Samaritan version; nay, even supposing that we now possessed the identical autograph of the book of Genesis by the hand of its author, a most important question remains,—what means do we possess of translating it?

In similar cases we avail ourselves of the works of the immediate predecessors and of the contemporaries of the writer; but here we are acquainted with no work of any predecessor,—of no writing of any contemporary; and we do not possess the works of any writers in the same language, even during several succeeding centuries, if we except some few of the sacred books. How, then, is it possible to satisfy our minds of the minute shades of meaning of words, perhaps employed popularly; or, if they were employed in a stricter and more philosophical sense, where are the contemporary philosophical writings from which their accurate interpretation may be gained?

The extreme difficulty of such an inquiry will be made apparent by imagining a parallel case. Let us suppose all writings in the English, and, indeed, in all other languages previous to the time of Shakspeare, to have been destroyed,—let us imagine one manuscript of his plays to remain, but not a vestige of the works of any of his contemporaries; and, further, suppose the whole of the succeeding works of English literature to be annihilated nearly up to the present time. Under such circumstances, what would be our knowledge of Shakspeare? We should undoubtedly understand the general tenor and the plots of his plays. We should *read* the language of all his characters; and, viewing it generally, we might even be said to understand it. But how many words connected with the customs, habits, and manners of the time must, under such circumstances, necessarily remain unknown to us! Still further, if any question arose, requiring for its solution a knowledge of the minuter shades of meaning of words now long obsolete, or of terms supposed to be used in a strict or philosophical sense, how completely unsatisfactory must our conclusions remain? Such I conceive to be the view which common sense bids us take of the interpretation of the book of Genesis. The language of the Hebrews, in times long subsequent to the date of that book, may not have so far changed as to prevent us from rightly understanding, generally, the history it narrates; but there appears to be no reasonable ground for venturing to pronounce with confidence on the minute shades of meaning of allied words, and, on such foundations, to support an argument opposed to the evidence of our senses.

I should have hesitated in offering these remarks respecting the right interpretation of the Mosaic account of the creation, had the argument depended on any acquaintance with the language in which the Sacred Volume is written, or on any refinements of criticism, had I possessed that knowledge; but, in estimating its validity, or in supplying a more cogent argument, I entreat the reader to consider well the difficulties which it is necessary to meet.

1st, The Church of England, if we may judge by the writings of those placed in authority, has hitherto considered it to have been expressly stated in the book of Genesis, that the Earth was created about six thousand years ago.

2dly, Those observers and philosophers who have spent their lives in the study of geology, have arrived at the conclusion, that there exists irresistible evidence that the date of the Earth's first formation is far anterior to the epoch supposed to be assigned to it by Moses; and it is now admitted by all competent persons, that the formation, even of those strata which are nearest the surface, must have occupied vast periods,—probably millions of years,—in arriving at their present state.

3dly, Many of the most distinguished members of the Church of England now distinctly and formally admit the fact of such a lengthened existence of the Earth which we inhabit, for it is so stated in the eighth *Bridgewater Treatise*, a work written by the Professor of Geology in the University of Oxford—himself holding an office of dignity in that church, and expressly appointed to write upon that subject by the Archbishop of Canterbury and the Bishop of London.

4thly, The Professor of Hebrew at the same university has proposed a new interpretation of those passages of the book of Genesis which were hitherto supposed to be adverse to the now admitted facts.*

Such being the present state of the case, it surely becomes a duty to require a very high degree of evidence before we again claim authority for the opinion, that the book of Genesis contains such a precise account of the work of creation, that we may venture to appeal to it as a refutation of observed facts. The history of the past errors of our Parent Church supplies us with a lesson of caution which ought not to be lost by its reformed successors. The fact, that the venerable Galileo was compelled publicly to deny, on bended knee, a truth of which he had the most convincing demonstration, remains as a beacon to all after time, and ought not to be without its influence on the inquiring minds of the present day.

If the explanation offered by the Professor of Hebrew be admitted, those who adhere to it must still have some misgivings as to the effect of new discoveries in nature causing continual occasion for amended translations of various texts; whereas, should the view which has been advocated in this chapter be found correct, instead of fearing that the future progress of science may raise additional difficulties in the way of revealed religion, we are at once relieved from all doubt on that subject.—*Babbage's Bridgewater Treatise*, p. 63. Ed. New Philos. Jour.

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* V. Buckland's Bridgewater Treatise, p. p. 22—5.

The November-Asteroïds.

Notice of a recent Communication relating to them, by Dr. OLBERS.

Those who take an interest in physical sciences have, within these few years, had their attention directed to the periodical return of shooting-stars; and recent observations go far to subvert the opinions formerly held with respect to them—namely, that they originated within the limits of our atmosphere. On the contrary, it is now looked upon as an established fact, that they are heavenly bodies of inconsiderable dimensions, but which, in common with all others of a similar nature, have, of course, a regular motion. They are, in short, supposed to represent, in the solar system, the animacules of the animal kingdom.

Olbers, in the *Jahrbuch fur* 1837, brings forward, with irresistible force and clearness, numerous proofs in favour of this supposition; and the following general sketch will, it is hoped, give a faithful abstract of the arguments to which he therein has recourse, in favour of the new theory.

On the 12th of November, 1799, before sun-rise, Humboldt and Bonpland, on the coast of Mexico, observed, during four hours, a succession of many thousand shooting-stars and small fire-balls. The part of the heavens whence they emanated lay due east, and they extended over a space of 30° to the right and left of that point. They rose above the horizon in an east-north-east direction, and fell towards the south, describing unequal arcs in their course. Some attained an altitude of 40°, and there were none that did not rise to between 25° and 30°. Several appeared to burst; the largest, however, disappeared, without throwing out any coruscations. Some had a large nucleus equal to Jupiter in brightness, and all, or nearly so, had tails.

This remarkable phenomenon was also simultaneously observed from various distant points of the earth. A similar display was again witnessed on the night of the 13th of November, 1831, on the night of the 12th of November, 1832, on the same night in 1833, and on the night of the 13th of November, 1834.

This periodical return induced many persons, where the cloudy state of the weather did not altogether put a stop to their observations, to keep a good look out on the nights between the 11th and 14th of November, 1836; nor was the trouble they thus took thrown away, for it appears that a vast number of shooting-stars were actually at that time observed from the most distant points of the globe.

The periodical return of these bodies would thus seem to be established, but this is not sufficient for us to form any just notions as to their nature. The first step to be taken to that end is to discover what their elevation above the earth's surface may be.

In order to arrive at this, two observers in concert determine the angle under which they see a shooting-star, indicating likewise the time of its appearance, and noting the star near which it came into sight. Now, if two such observers remark at the same instant, and in the same direction and position, any shooting-star, it may fairly enough be assumed that they are looking at one and the same object. A triangle is thus obtained; the base—that is to say the distance between the observers, and two angles of which, are approximately known, so that the calculation of the distance presents no difficulties whatever.

Simple as these observations are, it is evident that they cannot pretend to any thing like exactness; as they are, however, very numerous, their errors are thereby eliminated, and approximate results, at least, are arrived at; and the distance being thus ascertained, their real velocity may be deduced from the apparent.

Olbers conveys his opinions respecting these bodies in the following terms. The remarkable and very unequal distribution of those small bodies revolving round the sun, which form the fire-balls and shooting-stars of our planetary system, as well as the general resemblance, and nearly similar nature, of all the meteoric stones which fall from time to time, (taking into consideration both their external characters, and also their chemical composition,) seem to indicate not only that they have one common origin, but also that it was one common cause which has thus hurled them into space.

One cannot help reverting, involuntarily, to the hypothesis which holds the four new planets, Vesta, Juno, Pallas, and Ceres, to be but fragments of a larger planet, formerly revolving round the sun between Mars and Jupiter, but which has been shattered to pieces by some violent catastrophe. On such a planet as this exploding, and being scattered about in all directions there would also be, independent of the larger fragments, innumerable much smaller, indeed minute pieces, hurled into space, and these would now circulate about the sun in ellipses of various degrees of eccentricity.

It must be borne in mind that Olbers treats this, however, as a mere hypothesis and distinctly says he by no means adopts the language of Professor Wildt, who, as may be seen by referring to the 9th Volume, p. 408, of *Voigt's Magazin fur den neusten Stand der Naturkunde*, unreservedly asserts, the stones which have fallen on the earth to be "the ruins of some globe which has been destoyed, and which revolve round the sun till, sooner or later, they fall in with a planet.

"They without doubt, he continues, belong to the group of Ceres, Pallas, &c., and thus we see how it is that their appearance and composition bear such general resemblance to each other."

What we really know of shooting-stars, of those, at least, which are allied to fire-balls, if not actually identical with them, may be summed up in the following words:—

1st. They move at considerable elevations above the earth, their distances from it ranging from between 140 to 190 miles.

2ndly. Their velocity is equal to that of several of the planets; and their relative velocity, compared with the earth, may vary from 37 to 42 miles in a second.*

3rdly. Hence it is from without the regions of space that they enter our atmosphere, and it is not within the limits of the latter that they originally take their rise.

4thly. They are not ejected into the earth from the moon.

Much that is connected with these interesting meteors, it is, however, quite out of our power to explain. For instance, how is it they become ignited? How is it that, in the extremely rare medium in which they move, they continue glowing, or, indeed, burning with such brilliancy? How can the comparatively small mass of the meteoric stones which falls to the earth expand in those elevated regions to bodies several hundred feet in diameter?

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* Littrow gives the mean velocity of Mercury only at 30 miles per second, that of the earth at 19 miles, and the mean of the four new planets at 12.

Must we not suppose, with the acute Von Hoff, of Gotha, that the materials of which shooting-stars originally consist are submitted to a peculiar chemical process upon entering into our atmosphere, the result of which is the formation of the substance we see fall as meteoric stones,—a process which surely cannot consist merely of fusion?

All these questions, and many others of a similar nature, will possibly remain for ever unanswered, or be, at least, but unsatisfactorily solved; for it scarcely appears probable that time can give us a deeper insight into the circumstances connected with this subject. How can we, indeed, even hope to arrive at a satisfactory knowledge of processes carried on at altitudes where the air is far rarer than in the most perfect vacuum we can produce, and where, moreover, the atmosphere possibly consists of gases of which we know nothing, and all carried on at a temperature of absolute cold. What powers, affinity, electricity, and magnetism are there invested with, will always remain a mystery to us. Ω.

Munich, 10*th July*, 1837.

Mag. Pop. Sci.

St. Elmo's Fire seen in Orkney.

During last February, 1837, (Sunday 19.) in a tremendous gale, my large boat sunk, and it was late on Tuesday night before we could get her up and drawn to the shore, after which we had to wait till three o'clock next morning till the tide ebbed from her; she was during this time attached to the shore by an iron chain, about 30 fathoms long, which did not touch the water, when, to my astonishment, I beheld a sheet of blood-red flame, extending along the shore for about 30 fathoms broad and 100 fathoms long, commencing at the chain and stretching along the shore and sea in the direction of the shore which was E. S. E., the wind being N. N. W. at the time. The flame remained about ten seconds, and occurred four times in about two minutes. Whilst I was wondering not a little, the boatmen, who, to the number of twenty-five or thirty, were sheltering themselves from the weather, came running down apparently alarmed, and asked me if I had ever seen anything like this before. I was about to reply, when I observed their eyes directed upwards, and found they were attracted by a most splendid appearance at the boat. The whole mast was illuminated, and from the iron spike at the summit, a flame of one foot long was pointed to the N. N. W., from which a thunder-cloud was rapidly coming. The cloud approached, which was accompanied by thunder and hail; the flame increased and followed the course of the cloud till it was immediately above, when it arrived at the length of nearly three feet, after which it rapidly diminished, still pointing to the cloud, as it was borne rapidly on to S. S. E. The whole lasted about four minutes, and had a most splendid appearance. I regretted afterwards that I was so occupied with the flame at the mast head that I did not observe whether the red flame on the ground continued during the time the cloud was passing.—*Extract of a Letter from William Traill, Esq., Kirkwall, to Professor Traill, dated May* 16, 1837.

Ed. New Philos. Jour.

Simultaneous Meteorology.—No. II.

Table of Hourly Meteorological Observations, made during the 21st and 22nd June, 1837, at BLACKHEATH ROAD, near Greenwich, about four miles and a half S. E. of London, *by and under the superintendence* of J. H. BELVILLE, in conformity with the instructions circulated by the South African Literary and Philosophical Institution.

DATE.	HOUR.	BAROMETER.	THERM.		WIND.		Pro. of cloud.	STATE OF THE ATMOSPHERE, CLOUDS, &c.
			Attached.	In open air.	Direction.	Strength.		
		Eng. In.	Fr. °	Fahr. °				
Wednesday, June 1837.	VI. A.M.	30.028	67	57.1	W.	1	10	*Stratus.* The *minimum* of the self-registering thermometer before sunrise, 54.5°.
	VII.	30.046	67	59.0	W.	1	10	Ditto.
	VIII.	30.052	68	62.8	W.	1	5	*Stratus* into *cumulus.*
	IX.	30.054	68	67.8	W. by S.	1	5	*Cirro-cumulus.* Hazy.
	X.	30.075	69	69.0	W. by S.	1	4	*Cumulus.*
	XI.	30.085	70	67.0	S.W.	1	3	Ditto.
	XII.	30.085	71	69.9	S.W.	2	3	Ditto. Brisk wind and cool.
	I. P.M.	30.098	72	70.5	S.W.	2	3	Ditto. Ditto ditto.
	II.	30.098	73	73.4	W.S.W.	1	6	Ditto.
	III.	30.100	73	73.6	W.S.W.	1	7	The same as before.
	IV.	30.103	72	71.2	W.S.W.	1	8	Dense *cumulo-stratus* has prevailed since last observation.
	V.	30.110	71	71.1	W.	2	5	Getting clearer and lighter.
	VI.	30.134	72	69.1	W. by S.	2	5	Getting more clear.
	VII.	30.150	74	68.2	S.S.W.	1	6	*Cirro-cumulus* to the S.
	VIII.	30.164	73	66.5	S.W.	1	8	Ditto ditto collecting. [&W.
	IX.	30.176	72	62.9	S.W.	1	1	Getting clearer again towards the N.
	X.	30.198	71	60.6	W.S.W.	1	4	Clouds again collecting to N. of zenith
	XI.	30.200	71	59.5	W.S.W.	0	4	Nearly cloudless. *Moon rises in a clear horizon.*
	XII.	30.205	71	58.0	none	0	1	Cloudless. Calm and serene throughout the night.
Thursday, June 22.	I. A.M.	30.210	71	54.5		0	0	Ditto.
	II.	30.231	71	53.6		0	0	Do. A low creeping mist on the distant hills to the S. a little before sunrise. The *maximum* of the self-registering thermometer at sunrise, 50.0°.
	III.	30.240	71	53.0		0	0	Do.
	IV.	30.246	70	51.7	W.	0	0	Do.
	V.	30.250	70	54.0	W.	0	0	Do.
	VI.	30.262	69	55.8	W.	1	0	Do.
	VII.	30.281	69	59.0	S.S.W.	0	2	Light *cirai.* A breeze coming on.
	VIII.	30.293	68	63.5	N.W.	0	3	Great obscurity from mist and smoke.
	IX.	30.296	68	66.0	N.	1	1	Wind changes to N. Mist increases.
	X.	30.298	69	70.0	N.	1	1	A few *cumuli* appear through the mist.
	XI.	30.300	70	71.5	S.W.	1	2	So misty that the sun scarcely pro-
	XII.	30.301	71	73.0	W.	1	3	*Cumuli* & *cirro-* [duces a shadow.
	I. P.M.	30.301	71	69.0	N.	1	4	Ditto. [*cumili.* Mist and smoke still prevail.
	II.	30.302	72	72.0	N.E.	1	3	Ditto.
	III.	30.300	72	71.0	N.E.	0	4	Ditto.
	IV.	30.300	72	71.3	E.	0	5	Clouds and mistiness rather going off.
	V.	30.298	73	71.2	E.	0	4	A little clearer.
	VI.	30.300	72	69.8	E.	0	4	No variation since the last observ.

NOTES.—The two days rather cloudy and misty; the intervening night clear and serene. The whole of the clouds came up from the *westward.* The *mist* prevailed chiefly in the *lower* atmosphere.

A little before midnight, a large fire broke out to the N. W. between Deptford and

Progress of Civil Engineering.

Telegraphic Communication betwixt London and Edinburgh.

From the Scotsman.

It has been found by experiments made with a view to ascertaining the velocity of electricity, that it is transmitted instantaneously, by means of a common iron wire, a distance of eight miles; and electricians of the first eminence have declared their opinion that, judging from all scientific experience, the electric or galvanic influence would be almost instantaneously transmitted from one end to the other of a metallic conductor, such as ordinary copper` wire of moderate thickness, of some hundred miles in length.

If this scientific theory is correct, it follows that a wire, secured by a coating of non-conductors, and protected from external influence or injury, and laid under the turnpike-road between Edinburgh and London, could be the means of distinctly indicating to a person stationed in London, that such wire had been electrified or galvanized in Edinburgh—the transmission of the electric or galvanic influence being clearly discernible by various well known means.

How, then, is this scientific fact to be applied to purposes of practical and general utility? Simply by laying as many wires separated from each other as will correspond to the letters of the alphabet, and preconcerting between the persons stationed at two extremities of the line of communication, that each individual wire is to represent a particular letter; because, if the person stationed in Edinburgh can, by applying the electric influence to any one wire, instantaneously apprize another person stationed in London that a particular letter of the alphabet is thereby indicated, words and sentences *ad infinitum*, may be communicated, and the idea of a perfect telegraph would be realized.

Without experience it is impossible to say with what rapidity this electro-magnetic telegraph could be worked: but, in all probability, intelligence could be conveyed by such a medium as quickly as it is possible to write, or at least to print; an apparatus could be constructed somewhat resembling the keys of an organ, by which the letters of the telegraph could be touched with the most perfect ease and regularity.

It has been mentioned, that the transmission of the electricity or galvanism could be discernible by various means well known. If any indication,

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Rotherhithe, at a spot about two miles in a direct line from the Meteorological Station, from which a dense column of smoke rose nearly perpendicular to a great elevation. *No perceptible current of air could be detected.* The night ended remarkably still and calm.

The time was taken from a good clock, keeping mean time, *rate* scarcely perceptible during the observations; *error* obtained by observation of the Ball-Drop, Royal Observatory, Greenwich.

The Barometer has an elevrtion of about 40 feet above the River Thames.—Makers, Watkins and Hill, London; reads off about .025 lower than a standard barometer lately made by the same artists. The Thermometer has a northern aspect—Maker, Dollond.

The *strength* of the wind is indicated by the figures, thus—0 means *no wind perceptible;* 1, *very light breeze*; 2, *strong breeze.*

In the column headed *Proportion of cloud,* 0 signifies *quite clear;* 10, *no blue sky visible; 5, sky half covered,* &c.

however slight, is made, that is enough, all that is wanted being that it should be perceivable by the person placed to watch the telegraph.

It has been assumed, that the electric current is capable of transmission by means of a single impulse from Edinburg to London. But it is not indispensible that so great a distance should be accomplished at once. Intermediate stations for supplying the telegraph with new galvanic influence could be resorted to, and its perfect efficiency still preserved.

The best mode of troughing or protecting the metallic conductors, and separating them both from each other, and from the surrounding substances by which the electric or galvanic influence might be diverted, would, of course, require considerable scientific and mechanical skill; but the object appears perfectly attainable. Insulating or non-conducting substances, as gumlac, sulphur, resin, baked wood, &c., are cheap; and the insulation might be accomplished in many ways. For example, by laying the wires, after coating them with some non-conducting substances, in layers betwixt thin slips of baked wood, similarly coated, the whole properly fastened together and coated externally. These slips might be perhaps ten yards long, and at the joinings precautions for the expansion and contraction of the wire by the change of temperature, might be adopted. The whole might be enclosed in a strong oblong trough of wood, coated within and pitched without, and buried two or three feet under the turnpike road.

The expense of making the telegraph proposed is, of course, an important element in the consideration of its practicability and utility.

The chief material necessary, viz., copper wire, is by no means expensive. It is sold at 1*s.* 6*d.* per pound, of sixty yards in length. The cost of a wire from Edinburg to London, say 400 miles, would thus be about 900*l.*; but say for solderings, &c., 100*l.* additional, or that each copper wire, laid from Edinburg to London, would cost 1,000*l.* sterling, and that the total expense for the wires necessary to indicate separately each letter of the alphabet, would be 25,000*l.* The purchase of so large a quantity would, of course, be made at a considerably less price; but probably one or two additional wires might be needed, and the circuit of the electrical influence must be provided for by one or more return wires.

The coating, separating, and troughing of the wires can be accomplished by low-priced materials, and the total expense of the whole work (except the price of the wires,) allowing a large sum for incidental expenditure, has been roughly estimated at 75,000*l.*, making a maximum expenditure of, say, 100,000*l.* for the completion of the telegraph. For a proportional additional sum it might be extended to Glasgow.

As to the working of the telegraph, it is apprehended, that even if the speed of writing were not attained, there could at least be no difficulty in indicating one letter per second. At this rate, a communication which would contain sixty-five words would occupy about five minutes. This is supposing the vowels to be all indicated. But abbreviation in this, and many other respects, would no doubt be contrived; and the number of words in the communication supposed, are greater than necessary for an ordinary banking or commercial letter, or for friendly inquiries and responses. Supposing, however, that each communication was to occupy five minutes, and to be charged five shillings each, if the telegraph was worked twelve hours a-day (that is, six hours from each end,) it would produce a revenue of 36*l.* daily, or 10,800*l.* per annum, supposing there were 300 working days in the year. If, however, the plan is practicable, the public intelligence that would, no doubt, be transmitted by the telegraph would be sufficient to keep it in operation night and day.

Arrangements are being made for having the necessary experiments tried on a metallic conductor of fifty or a hundred miles in length, and if the same instantaneous and perfect indication of the passage of the electric or galvanic fluid is found to take place, as in the case of the recent experiments at the University, the triumph of the scheme would be complete.

Mech. Mag.

Captain Ericsson's new Propeller.

The American packet ship "Toronto" of 630 tons burthen, and drawing 14 feet 6 inches water, was on Saturday last towed down the Thames at the rate of full 4½ knots an hour *against* wind and tide by an experimental steam-boat, called the "Francis B. Ogden," (after Mr. Ogden, the able and intelligent Consul of the United States, at Liverpool,) fitted with the new propelling apparatus lately invented and patented by Capt. Ericsson. The "Francis B. Ogden" measures 45 feet in length, 8 feet beam, and draws 2 feet 3 inches water without the keel. The propelling apparatus is placed at the stern, and works entirely under the water. It consists of a peculiar application of the old, and well known principle of the water screw, by which a great propelling power is concentrated in a small space. Of the degree of power concentrated, no better proof can be adduced, than the fact, that the speed of 4½ knots against wind and tide, was produced by an apparatus, measuring only 5 feet 2 inches in diameter, and 2 feet 2 inches wide, and worked by a high pressure engine having two cylinders of 14 inches stroke, and 12 inches diameter; and which, during the experiment, made only 60 strokes per minute, and showed a pressure of not more than 50lbs. to the square inch.

The new propelling apparatus consists of two short cylinders made of thin wrought iron, and supported by arms of a peculiar form, which are placed entirely under the water at the stern, and made to revolve in contrary directions round a common centre. To the outer periphery of each cylinder, there is attached a series of spiral planes or plates, which may, we understand, be placed at any desired angle according to the effect sought to be obtained, whether it be great speed, or great propelling power.

The apparatus may be made to ship and unship at pleasure; the engine that works it may also be loco-moveable, so as to be worked upon deck, and on any part of the deck, and in these two peculiarities, we are inclined to think the chief advantage of this new step in steam navigation will be found to consist. Sailing vessels may, by this means, command all the aid that steam can give them, without divesting themselves of any of their peculiar fitness for long sea voyages, or undergoing any change in their original construction.

We subjoin a copy with which we were favoured of the certificate given by the pilot and mate of the "Toronto" of the performance of the "Francis B. Ogden" on this occasion.

Packet Ship Toronto, in the Thames, 28th May, 1837.

We feel pleasure in certifying that your experimental steam boat, the "Francis B. Ogden," has this morning towed our ship at the rate of 4½ knots an hour, through the water and against the tide.

(Signed) E. Nashly, Pilot.
H. R. Hovey, Mate.

To Captain Ericsson.

Ibid.

Liverpool and Manchester Railway.

The cost of working the Liverpool Railway for the year 1836, (if the reports published by the directors be correct,) was, deducting interest paid on borrowed money, 133,225*l.* 7s., or at the rate of upwards of 420*l.* per working day. Of this sum 41,952*l.* 5s. 8d. is stated to be the cost of locomotive power for the thirty miles of railway on which locomotive engines are alone employed. The cost of locomotive power was, therefore, just 1400*l.* a mile.

While the facts above stated are startling to parties who believe in and promulgate the doctrines of the cheapness of railway formation and maintenance, what a light do they not throw on the profound wisdom of "Parliamentary private bill legislation?"

About 30,000*l.* was expended by the original promoters of the Liverpool Railway in order to prove to Parliament that it could be easily completed at a cost of 510,000*l.*, or about 16,000*l.* per mile, as stated in the first act of incorporation. Since that period nearly as much more has been expended in Parliament in obtaining new acts, which demonstrate that the railway will cost more than three times 16,000*l.* or 50,000*l.* sterling per mile, and that the original proof was an entire fallacy.

Nor is this instance a solitary one. About three years ago, the promoters of the London and Birmingham Railway were compelled to spend 50,000*l.* in proving to Parliament that it could be formed for 2,500,000*l.*, or about 22,000*l.* per mile. The London and Birmingham Railway Company are now before Parliament, proving it must cost 4.500,000*l.*, or about 40,000*l*, a mile, and this at a period when the undertaking is little more than half finished. In the course of a year or two this estimate will no doubt be raised to the Liverpool Railway standard of 50,000*l.*

To work this railway at Liverpool railway prices will cost about 500,000*l.* a year, or about 1600*l.* per day, of which 150,000*l.* will be for locomotive power alone.

It is needless to multiply examples—it is sufficient to say that many thousand pounds are now in progress of being expended every week before Parliament, in proving that lines of railway can be completed through difficult tracts of country at from a third to a fifth of the cost which experience has demonstrated as necessary to construct a perfect modern railway, and that when completed they can be worked and maintained for a half and a third of the cost per mile now expended on the Liverpool Railway.

Min. Jour.

Nothwithstanding this enormous cost and annual expenditure, we find by the last quotations of the price of stocks, that the Liverpool and Manchester Railway stock is 206*l.* for 100*l.* and that of the London and Birmingham, 130*l.* for 85*l.* paid. The former, (it is stated in a late number of the Liverpool Times,) is now paying ten per cent, and has never paid less than eight. The latter is computed, in consequence of a greater thoroughfare, to yield a clear profit of at least thirteen per cent on the original shares.

G.

Health of Tunnels.

Report on the Primrose-hill Tunnel on the London and Birmingham Railway, by John Paris, M. D., Thos. Watson, M. D., Cantab., Wm. Lawrence, Esq., and R. Phillips, Esq.

We the undersigned, visited together, on the 20th February, 1837, the Tunnel now in progress under Primrose-hill, with the view of ascertaining the probable effect of such Tunnels upon the health and feelings of those who may traverse them.

The Tunnel is carried through clay, and is lined with brickwork. Its dimensions, as described to us, are as follows: height 22 feet, width 22 feet, length 3,750 feet. It is ventilated by five shafts, from 6 to 8 feet in diameter, their depth being 35 to 55 feet.

The experiment was made under unfavourable circumstances. The western extremity of the Tunnel being only partially open, the ventilation is less perfect than it will be when the work is completed. The steam of the locomotive engine also was suffered to escape for twenty minutes, while the carriages were stationary near the end of the Tunnel; even during our stay near the unfinished end of the Tunnel, where the engine remained stationary, although the cloud caused by the steam was visible near the roof, the air for many feet above our heads remained clear, and apparently unaffected by steam or effluvia of any kind; neither was there any damp or cold perceptible.

We found the atmosphere of the Tunnel dry, and of an agreeable temperature, and free from smell or perceptible effluvia of any kind; the lamps of the carriages were lighted; and, in our transit inwards and back again to the mouth of the Tunnel, the sensation experienced was precisely that of travelling in a coach by night, between the walls of a narrow street. The noise did not prevent easy conversation, nor appear to be much greater in the Tunnel than in the open air.

Judging from this experiment, and knowing the ease and certainty with which thorough ventilation may be effected, we are decidedly of opinion that the dangers incurred in passing through well-constructed Tunnels are no greater than those incurred in ordinary travelling upon an open railway or upon a turnpike-road; and that the apprehensions which have been expressed that such Tunnels are likely to prove detrimental to the health, or inconvenient to the feelings of those who may go through them, are perfectly futile and groundless.

John Paris, M. D.

Thomas Watson, M. D., Cantab., Physician to the Middlesex Hospital, and Professor of Medicine at King's College.

William Lawrence, Surgeon to St. Bartholomew's Hospital.

Rd. Phillips, Lecturer on Chemistry at St. Thomas's Hospital.

London, Feb. 21, 1837. Mech. Mag.

Russian Column.

A Column has recently been erected to the memory of Alexander 1. in Petersburg, in the Place ——, of which the following are the dimensions:—Shaft of the pillar, in one piece of granite, length, 84 feet; diameter, 12½ feet; weight 350 or 400 tons. The bronze figure on the top is 15 feet high, independent of the cross, or 23 feet with the cross; weight, 13 tons. The bas-reliefs are in four plates, and the length of each 17½ feet; the height being 10½ feet. Some of the figures stand out nearly 2 feet. The weight of the four plates is 44 tons; entire height of the monument, 154 feet; weight of brass, 158 tons. The erection of this monument cost 10,000,000 of rubles, or £400,000 sterling; and it was erected in two years and a half. The stone pedestal on which the whole stands is 28 feet square, and 7 feet hick. Architec. Mag.

Mechanics' Register.

English Patent-Law Grievance.

The inventors of this country, and the introducers of inventions of other countries into this, were obliged to pay down to the attorney-general and other agents, &c., of the government during the past year, above £42,000. *What did the attorney-general,* &c., *in return to them for this vast and oppressive extortion ?*

If we look a little further back, the following exhibition of this legal wrong will then present itself.

TOTAL SUMS paid by Inventors, in Ten Years, ending December 1834, according to the Returns made, and calculations of extras and occasional charges.

	£.	s.	d.	Patents	£
English patents per report . . .	106	11	8		
Stamp duty on specification, average . .	8	0	0		
Official charge of drawing, do. do. . .	5	5	0		
Extras, joint names, private seals, journeys, expedition caveats, attendance in opposition, &c. &c.; *exclusive* of drawings and extra charges of the patent solicitor	12	3	4		
	132	0	0	×1,684	=222,288
Stamp Duty on Warrants, Colonies . . ,	6	0	0	×1,684	= 10,104
Scotch patents, per report	79	10	5		
Stamp duty on specifications, average . . .	8	0	0		
Charge of drawing, do. do.	5	5	0		
Extras, joint names, &c. &c. . . . . ,	7	4	7		
	100	0	0	× 489	=48,900
Irish Patents, per report	128	5	11		
Stamp duty on specification, average . . .	4	0	0		
Drawing, do. do.	5	5	0		
Extras, joint names, &c. &c.	7	9	1		
	145	0	0	× 207	=30,015
Colonies, at additional charge with extras, average	7	10	0	× 300	= 2,240
Total amount paid by inventors in ten years, on 2380 patents .					£313,557

MACKINNON. *Speech in the House of Commons, Feb.* 14, 1837, *Appendix.*

Three hundred and thirteen thousand, five hundred and fifty-seven pounds, "paid by inventors in ten years," *exclusive*, it will be observed, of the drawings and charges of the patent-solicitors on 2380 patents.

The penalties inflicted on the inventive genius of Britain during the *present* year, up to the 25th ult, in the shape of government stamps and fees on patents, amount to more than £11,000!

N. B. This sum has been paid in *ready money* on taking the first steps, and as many of the inventors are poor men, (*Operatives,*) and a great many others of them persons to whom it would be very inconvenient to pay at least £100 down, they have been obliged to go into debt, or mortgage or dispose of their inventions, either wholly or in part, &c. Mag. Pop. Sci.

Preservation of Mineral Specimens from decomposition.

Several questions have been brought forward, as to the best method of preserving from decomposition fossil fruits, wood, &c.; and various methods have been proposed with a view of attaining that object. It has been suggested, that after washing with turpentine and lamp-oil, a thin coating of isinglass in solution may be advantageously employed. On the 20th of last September, a friend brought me some fossils from Sheppey in pyrites, of which one specimen in particular was more affected by decomposition than the others. I immediately, after cleaning, gave them a dressing of turpentine and lamp-oil, recommended by Mr. Hill, and laid them in the open air to dry; but, in only two days after this, I found decomposition beginning afresh, the sulphate of iron forming upon them. I removed them into the house, wiped the lamp-oil clean off, and then gave them a coating of dissolved isingless and spirits of wine; and, at the end of seven months after this simple process, I find that they are perfectly free from the slightest appearance of decomposition; and have every reason to conclude that, for the purposes of the cabinet at least, this process for their preservation will be found sufficient. On applying the lamp oil, I found it of little or no use in this instance: it will not dry, it fills up the fibrous appearance on the outside of the fossils, and almost destroys their beauty. The quality of the isinglass obviates this completely; and it dries almost instantaneously.

Mag. Nat. His.

Fossil Infusoria.

Fossil infusoria have been eaten in Lapland in times of great scarcity. M. Retzius, Professor of Anatomy at Stockholm, has recently written thus to M. Ehrenberg:—"Through the kindness of M. Berzelius, I have received some fragments of the siliceous deposit of Franzensbad, sent by you to him. The carapaxes of the fossil infusoria contained in this deposit, made me think of a mineral substance vulgarly called Bergmehl (flour of the mountains,) analyzed and described by M. Berzelius in the Annales de Poggendorf, for the year 1733. This flour of the mountains contains silex, an animal substance, and the *crénique* acid *(quellen saure,)* discovered by this great chemist. The Laplanders mix the bergmehl, when a famine takes place, with the flour of corn and bark, in order to make bread. They fed in this manner in 1833, in the little district of Degerfors, on the frontiers of Lapland, in the sixty-fourth and sixty-fifth degrees of latitude. On examing the mountain flour, which is considered by these superstitious people to be a gift from the great spirit of the forests, I have discovered nineteen different forms of infusoria, with siliceous carapaxes, of which I send you drawings. The whole of the mineral is composed of them, and the conjecture which I formed on its analogy with the deposit at Franzensbad, proves to have been well founded. M. Ehrenberg has received the mineral flour from Lapland and several of the infusoria which it contains are supposed to be still living near Berlin. The infusoria are *eaten* in Degerfors—I do not say that the inhabitants are nourished by them."—*Baron de Humboldt.*

Min. Jour.

The endless Ladder.

A patent has recently been obtained for a most ingenious and useful machine, adapted to mining and many other purposes, where the main object is to raise or lower weights and packages in constant succession. This simple, but very effectual contrivance, consists of an endless ladder, made either of chain or rope, which passes *over* and *under* two revolving drums or cylinders, mounted upon horizontal axes; one placed at the bottom, and the other at the top, of a shaft or plain, to or from which the ladder is intended to reach. A continuous motion being given to either of the cylinders by the power of steam, or animal force, the endless ropes or chains, furnished with horizontal staves, like those of a common ladder, are made to circulate over the revolving cylinders by which they are extended, so that one part of this endless ladder is continually ascending with a slow but uniform motion from the lowermost of the cylinders to the uppermost, whilst *vice versa*, the other part of the ladder is descending to the lowermost in an uninterrupted circulation. A vast deal of labour is thus unremittingly performed, with the important result of great economy in time and power. The invention also provides a safe and easy conveyance for men; the accomplishment of which, in a philanthrophic, as well as any other point of view, has long been a desideratum in mining operations. For this purpose, a small movable step or footboard, furnished with a handrail, is applied, which, if desired, can be made wide enough to admit of several persons standing abreast, who are, by this means, passed up and down without fatigue, and in perfect security. Independently of the certain advantages that would result from the application of such machinery to the purposes for which it appears so admirably adapted, we consider Dr. Spurgin, (of London, the inventor of this apparatus,) to have thus planned a most admirable contrivance for the poor miners, a numerous class of our fellow citizens, who, from the peculiar nature of their occupation, are exposed to fearful risks of life and limb, and whose casualities would be materially diminished by the adoption of this machine. Min. Jour.

Simultaneous Magnetic Observations.

In accordance with the proposition of Prof. Gauss, simultaneous magnetic observations are now made on fixed days, at various places from Upsal to Palermo, of which Munich is the connecting point between the north and south, and, from the vicinity of the Alpine ranges, one of considerable interest. By the kind assistance of several of his friends and colleagues, Prof. Steinheil has been enabled to make regular observations during the two last of these "magnetic periods" of 24 hours each,—that is to say, on the 28th of November 1835, and the 30th of January 1836; the results had been communicated to Prof. Gauss by whom they would be laid before the public. He then showed the section a copy of observations thus made to Göttingen and Munich, graphically reduced; when thus compared, the similarity of the two curves was very remarkable, and evidently must have proceeded from causes operating simultaneously. Mag. Pop. Sci.

Animalculæ.

In the last published part of the new edition of the "Enclopædia Britannica," under the article microscope, by Sir David Brewster, we have the astounding information that "the size of a single individual of the animalculæ lately discovered in silicious rocks is 1-288th of a line, or 1-3456th of an inch. In the polishing slate from Bilin, in which there appear to be no vacuities, a cubic line contains, in round numbers, 23,000,000 of these animals, and a cubic inch contains 41,000 millions of them! The weight of a cubic inch of the polishing slate is 270 grains. There are, therefore, 187 millions of these animals in a single grain, or the silicious coat of one of these animals weighs the 187-millionth part of a grain." Min. Jour.

LUNAR OCCULTATIONS FOR PHILADELPHIA, JANUARY 1838.

Angles reckoned to the right or westward round the circle, as seen in an inverting telescope.
☞ For direct vision add 180° ☜

Day.	H'r.	Min.	Phase	Star's name.	Mag.	from Moon's North point.	from Moon's Vertex.
5	4	48	Im.	δ Arietis	4,	78°	25°
5	5	40	Em.			349	301
5	11	20	Im.	T′ Arietis,	6,	124	177
5	12	31	Em.			273	327
9	14	34.	N.	App. ☽ & 47Gem. ☽ N.1.′1	6,		
10	10	9	Im.	'ω Cancri	6,	74	18
10	11	37	Em.			263	227
10	11	37	N.	App. ☽ & ω^2 Cancri ☽ N. 1.′1			

Meteorological Observations for August, 1837.

Moon.	Days	Therm		Barometer.		Wind.		Water fallen in rain.	State of the weather, and Remarks.
		Sun rise.	2 P.M.	Sun rise.	2 P.M.	Direction.	Force.		
				Inches	Inches			Inches.	
⊗	1	67°	83°	29.80	29 83	W.	Moderate.		Clear—do.
	2	67	85	84	82	W.	do.		Clear—do.
	3	74	88	78	60	W.	Brisk.	.45	Clear—do.—shower.
	4	63	73	77	80	NW.	Moderate.		Cloudy—do.
	5	57	74	95	30.04	W.	do.		Clear—do.
	6	56	78	30.23	29	W.	Calm.		Clear—do
	7	63	76	15	10	E.W	do.	.36	Drizzle—cloudy.
	8	70	83	00	29.92	W.	Breeze.		Clear—do.
☾	9	74	84	29.70	64	W.	Moderate	.25	Cloudy—shower.
	10	72	78	64	66	NW.	do.		Clear—cloudy.
	11	69	74	75	81	E.	Brisk.	.19	Cloudy—rain.
	12	61	64	83	90	NE.	Moderate.	.20	Rain—cloudy.
	13	61	73	30 00	30.00	NE W.	Calm.		Cloudy—do.
	14	65	75	00	29.93	SW.	Moderate.		Fog—cloudy.
	15	63	74	29.98	96	E.SW.	do.		Cloudy—clear.
☉	16	64	73	91	91	E.SW.	do.		Cloudy—clear.
	17	65	76	90	85	SE.	do.		Fog—cloudy.
	18	69	82	85	85	SE.	do.		Cloudy—Clear.
	19	70	81	85	85	E.	do.		Lightly cloudy—do. do.
	20	68	72	75	72	E.	Blustering.		Cloudy—do.
☽	21	63	80	62	62	NW.W.	Moderate.		Clear—do.
	22	68	72	53	55	NW.	Brisk.		Clear—do.
	23	56	66	75	75	W.	do.		Clear—partially cloudy.
	24	57	69	80	80	W.SE	Moderate	.5	Partially cloudy—do. do.—rain.
	25	54	73	80	90	W.	do.		Clear—do.
	26	56	71	90	86	SE.	do.	.56	Clear—cloudy—rain.
	27	68	77	75	74	W.	do.		Cloudy—clear.
	28	58	69	80	80	W.	Brisk.		Clear—do
	29	57	76	86	86	W.	Moderate.		Clear—do.
⊗	30	68	86	66	66	W.	Brisk.		Clear—do.
	31	66	60	70	75	NE.	Moderate		Cloudy—rain.
	Mean	64.26	75 64	29 83	29.83			2.06	

Thermometer.			Barometer.
Maximum height during the month		88.00 on 3d.	30.29 on 6th.
Minimum	do.	54.00 on 25th	29.53 on 22nd
Mean	do.	69.95	29.83.

JOURNAL

OF THE

FRANKLIN INSTITUTE

OF THE

State of Pennsylvania,

AND

MECHANICS' REGISTER.

DEVOTED TO

Mechanical and Physical Science,

CIVIL ENGINEERING, THE ARTS AND MANUFACTURES,

AND THE RECORDING OF

AMERICAN AND OTHER PATENTED INVENTIONS.

DECEMBER, 1837.

Practical and Theoretical Mechanics and Chemistry.

On Hydraulic and Common Mortars. By General TREUSSART, *Inspecteur du Génie. Translated from the French by J. G. Totten, Lt. Col. of Eng. and Brevet Col. United States Army.*

(CONTINUED FROM P. 323.)

ART. IV. *Of Artificial Hydraulic Limes.*

I stated in the first article, that, for a long time, the property possessed by certain limes of hardening in water, was attributed to the presence of oxides of manganese and of iron; several very hydraulic limestones were, however, at last found that contained no oxide of manganese and very little iron. It was observed, at a later period, that almost all hydraulic limestone contained from one to three-tenths of clay. This led to the opinion that when a certain proportion of clay is disseminated in limestone, it combines, by calcination, with the lime, and imparts to it the property of hardening in water. I stated that Mr. Vicat, Engineer of roads and bridges, published, in 1818, an interesting memoir on hydraulic mortars, and that he announced that by reburning fat lime with a certain quality of clay, he obtained very good hydraulic lime. I was bound to state, also, that Dr. John, of Berlin, presented to the Society of Sciences of Holland, a memoir on the subject which was crowned in 1818, and published the following year. He gave the analysis of many common, and hydraulic limes, as well as of many ancient mortars, and he showed that the hydraulic property of lime is due to a portion of clay which combines with it by calcination, and he calls this clay the *cement* of the lime.

The Minister of War sent the memoir of Mr. Vicat to those places where public works were in progress, and directed the experiments announced by this Engineer, to be repeated. I was then occupied at Strasburg, in rebuilding the sluices and other badly constructed hydraulic works; and was using the Obernai hydraulic lime. I had caused trass to be brought from Andernach, having had occasion to employ it at the great works of Vésel in 1806, 1807 and 1808, and knowing its excellence in constructions under water. In the erection of the sluices at Strasburg, we composed the mortar of the Obernai hydraulic lime, sand and trass; but this last substance being dear, I had begun making some essays towards replacing it with cement, when the memoir of Mr. Vicat was sent to me. I caused the experiments given in that work to be repeated, in the first place by an officer of Engineers, who used a clay of which bricks are made in the environs of Strasburg: he obtained no satisfactory result. A second officer was directed to recommence the experiments, and he was not more successful than the first. I repeated them, then, myself, and I took great pains in making the mixture of clay and lime; they were then calcined and I obtained a result, but it was a very feeble one. I then began anew, using other argillaceous earths, richer in clay, (*plus grasses*) and I got much better results. The mortars made with these artificial hydraulic limes, are shown in the following table.

TABLE No. IX.

No. of the mortar.	Composition of the mortar.	No. of days required to harden in water.	Weights supported before breaking.
		days	lbs.
1	Fat lime, reburnt with 2-10 of brick earth, slaked to powder and measured in powder 1 Sand . . . 2 } 3	28	44
2	Same lime, reburnt with 1-10 of Holsheim clay . . . . 1 Sand . . . . . 2 } 3	30	106
3	Same Lime, reburnt with 2-10 of Holsheim clay . . . . 1 Sand . . . . 2 } 3	20	187
4	Same lime, reburnt with 3-10 of Holsheim clay . . . . 1 Sand . . . . 2 } 3		—22
5	Lime from white marble, reburnt with 1-10 of Holsheim clay . . . . . 1 Sand . . . . 2 } 3	35	55
6	Same lime reburnt with 2-10 of Holsheim clay . . . . 1 Sand . . . . 2 } 3	27	174
7	Same lime, reburnt with 3-10 of Holsheim clay . . . . 1 Sand . . . . 2 } 3	36	130
8	Fat lime reburnt with 2-10 of pipe clay measured in paste . . . . . 1 Sand . . . . 2 } 3	10	132
9	Same lime reburnt with 2-10 of Holsheim clay do. . . . . 1 Sand . . . . 2 } 3	10	216

Table No. IX.—(Continued.)

No. of the mortar.	Composition of the mortar.	No. of days required to harden in water.	Weights supported before breaking.
		days	lbs.
10	Same lime reburnt with 2-10 of Yellow Ochre do. . . . . . . 1 Sand . . . 2 } 3	15	90
11	Same lime, reburnt with 2-10 of "*Sanguine*" do . . . . . . 1 Sand . . . . 2 } 3	25	44
12	Same lime reburnt with 2-10 of Sufflenheim clay do. . . . . . 1 Sand . . . . 2 } 3	17	110
13	Same lime reburnt with 2-10 of Frankfort clay do. . . . . . 1 Sand . . . . 2 } 3	17	121
14	Same lime reburnt with 2-10 of pipe clay, but more highly calcined than the lime of No. 8 . . . . . . 1 Sand . . . . 2 } 3	16	197
15	Fat lime reburnt with 2-10 pipe clay, measured in paste . . . . . 1 Trass . . . . . 2 } 3	5	307
16	Lime from white marble, reburnt with 2-10 of alumine do. . . . . . 1 Sand . . . . 2 } 3		
17	Same lime reburnt with 1-10 of alumine and 1-10 of white sand do. . . 1 Sand . . . . 2 } 3		
18	Same lime reburnt with 2-10 of white sand pulverized do. . . . . 1 Sand . . . . 2 } 3		
19	Fat lime reburnt with 2-10 of white sand pulverized do. . . . . . 1 Sand . . . . 2 } 3	21	51
20	Same lime reburnt with 2-10 of common sand pulverized do. . . 1 Sand . . . . 2 } 3	28	35
21	Same lime reburnt with 2-10 of magnesia do. . . . . 1 Sand . . . . 2 } 3		
22	Same lime reburnt with several proportions of oxide of manganese do. . 1 Sand . . . . 2 } 3		
23	Same lime reburnt with several proportions of iron filings, and with oxide of iron in several states of oxidation 1 Sand . . . . , . 2 } 3		

Observations on the experiments of Table No. IX.

The hydraulic limes of this table were made by the process of Mr. Vicat: care being taken to temper the clays in water, and to make the mixture when they were reduced to the consistence of a homogeneous sirup.

If the clays be not diluted, the union with the lime cannot be so well made. The compound of lime and clay was made by taking one part of lime measured in paste, and the several proportions of clay mentioned in the table. All the limes were reburnt by placing them in Hessian crucibles, in the middle of the lime kiln; there was no exception but No. 14, which, as will be stated below, was more strongly calcined than the other limes. All the mortars were made as soon as the artificial limes were withdrawn from the kilns. The proportion of the first seven mortars was got by taking one part of lime in powder to two parts of sand. Some experiments having shown me that in this mixture there was too much sand, the mortars from No. 8 to No. 23 were made by taking one part of lime in paste to two parts of sand.

No. 1 consists of fat lime reburnt with $\frac{2}{10}$ of brick-earth of the environs of Strasburg: this did not give a good result, as may be seen in the last column.

Nos. 2, 3 and 4 were intended as a trial of the clay of the Village of Holsheim, near Strasburg. Lime was mixed, successively, with 1, 2 and 3 tenths of that clay: it will be seen that the best result was with $\frac{2}{10}$; the mixture in which there was $\frac{3}{10}$ of clay gave a mortar which did not bear the scale pan, and broke easily in the fingers. The Holsheim clay is reddish, and greasy to the touch. Further on, I will give its composition.

Nos. 5, 6 and 7 are a repetition of the three preceding experiments, with this difference—that lime made from white marble was employed, instead of the fat lime of the country. The greatest resistance that I obtained with this lime was in a mixture with $\frac{2}{10}$ of clay. It is remarkable that with $\frac{3}{10}$ of clay, I obtained a passable result, while in No. 4, with the same proportion of clay, and differing from No. 7 only in the kind of lime, the result was very bad. I do not know to what this can be owing.

No. 8 was made with a greasy and very white clay, brought from the neighborhood of Cologne, to Strasburg, to make pipes. This clay gave a tolerable result.

No. 9 is a repetition of No. 3: there is no difference except in the proportions. No. 3 was made by taking one part of lime when reduced to powder after the second burning, and two parts of sand, while for No. 9 one part of lime, when reduced to paste after the second burning, was taken, to two parts of sand: as there is more lime in a given bulk of paste than in an equal bulk of powder, this mortar contained less sand than No. 3, and it gave a better result. It may be seen by Tables No. II and III that the natural hydraulic limes of the environs of Strasburg, will not bear a large proportion of sand; and I think that, in general, hydraulic lime takes less than is commonly supposed.

No. 10 was made with the yellow ochre of commerce, and No. 11, with a substance known under the name of *sanguine:* the results were weak, especially No. 11.

No. 12 was made with the Sufflenheim earth, which is used in making refractory bricks that are highly esteemed in the country, and employed at the cannon foundry at Strasburg. No. 13 contained clay which is brought, because of its containing little or no iron, from the neighborhood of Frankfort, to Strasburg, to make alum. The results of these two trials are not very satisfactory.

No. 14 was made in the same manner, and same proportions as No 8;

it differs from it only in the lime being more highly calcined. The result was better. It is possible that each kind of clay requires a particular degree of calcination; this result may be owing also to this—that the lime and the clay contain very little iron, which appears to facilitate vitrification.

The lime of No. 15 was calcined like that of No. 8: but in lieu of making the mortar of sand, it was made of trass: the result was much superior.

No. 16 was made with $\frac{2}{10}$ of alumine mixed with lime from white marble. There was no result: the mortars had no sort of consistence.

No 17 was composed of lime from white marble, mixed with a factitious clay composed of equal parts of silex pounded very fine, and of alumine: this gave a better result. The bad result of No. 16 induced me, at first, to conclude that alumine alone had not the property of rendering lime hydraulic: but a mixture of silecious sand and alumine not having yielded a better result, although forming a factitious clay, I have thought it might be owing to the following circumstance: the alumine which I used was extracted from a quality of alum that I decomposed; this alum had been made of the Frankfort clay, of which I will give the analysis below. In making the alum, the clay is calcined before being dissolved in sulphuric acid. It is possible that the calcination takes away from the alumine the property of combining with lime in the dry way. We ought not, therefore, to conclude, from No. 16, that alumine has not the property of rendering lime hydraulic, but only, that, when it has been calcined, it has not that property. What makes me think that calcination deprived the alumine of the property of combining in the dry way with the lime, is, that I have calcined one part of fat lime with $\frac{2}{10}$ of puzzalona, without getting any result. To know if alumine has the property of rendering lime hydraulic, it will be necessary to use this substance before it has been calcined, or acted upon by any acid. Pure alumine exists in some countries: but it is rare: I had none at command.

Nos. 18 and 19 are, respectively, mixtures of marble lime and fat lime, with white sand, and common sand broken very fine. It is remarkable that I obtained a result with fat lime, and no result with the marble lime. I do not know to what to attribute this difference: it may happen perhaps in this way, that the fat lime, of which I shall give the analysis below, contains the red oxide of iron which might facilitate the union of the lime with the white sand, which is entirely silecious.

No. 20, made with fat lime, and with ordinary sand, which is granitic, and coloured by the red oxide of iron, gave a result nearly like No. 9. I regret not having made a fourth experiment with marble lime and ordinary sand, in order to prove, further, that oxide of iron facilitates the combination of silecious and granitic sands with lime, as appears to be inferable from a comparison of Nos. 18 and 19.

No. 21 was designed to ascertain the influence of magnesia: that which I used in this experiment had been calcined to disengage the carbonic acid. We see that no result was obtained. I repeated the experiments, using carbonate of magnesia in order to avoid the use of a substance which had already been calcined: the heat of the lime kiln would drive off the carbonic acid of the magnesia, so that the magnesia would be free to combine with the lime; I, however, got no result.

No. 22 was an attempt to ascertain whether manganese imparted any properties to the lime. I mixed, successively, one part of fat lime with one, two, and three tenths, of oxide of manganese, and calcined the mixtures: I did not observe that the lime had undergone the least change. It is quite astonishing that an opinion should have prevailed for so long a time, that it was to oxide of manganese, hydraulic lime owed its quality of hardening in water: mixing fat lime with this oxide would have shown that lime acquires no new property thereby.

No. 23 indicates that I made various experiments with iron. I took one part of fat lime, which I mixed successively with diverse proportions of filings of iron obtained from a locksmith's shop, and which I reduced to very fine powder. I did the same thing with black oxide of iron, with brown oxide at different degrees of oxidation, and with steel filings: all these mixtures were burned in a lime kiln and I then made mortars by uniting them with sand; the lime had acquired no hydraulic property; but I had occasion to remark that the oxide of iron had, nevertheless, a marked action on the lime. It is known that if we take lime made of white marble, and throw water on it as it comes from the kiln, it slakes at once, giving out much heat and vapour. In making the above experiments with iron, I remarked, that the higher it was oxidized the less heat and vapour were given out by the lime on throwing water upon it, and that it did not slake until after it had been wet for some time: the lime which had been calcined with $\frac{3}{10}$ of brown oxide of iron gave no vapour; I placed in it a thermometer of Réaumur, and at the end of half an hour it had risen but five or six degrees. The Boulogne pebbles contain, as I have observed, a considerable quantity of brown oxide of iron; it is owing to this, no doubt, that if water be thrown on these pebbles as they are taken from the kiln, they give very little heat and no vapour: they do not fall to powder, and must be reduced to that state by pounding. The fat lime of Strasburg, also, contains a certain quantity of brown oxide of iron; the limestone has rusty stains in many places: when water is thrown on it, after calcination, some time is required for it to begin to slake.

A great part of the clays used in the above table were analysed, under the direction of Mr. Berthier, at the school of mines. I give, below, the substances they contain; and also the analysis of the fat limestone of the neighborhood of Strasburg. This stone contains the following substances, viz: carbonate of lime, 0.964; oxide of iron, 0.020; water and loss 0.016. We see that after calcination, which drives off the water and the carbonic acid, there is left lime mixed with about two per cent of oxide of iron.

The following is the analysis made by Mr. Berthier of the different clays used by me.

	CLAYS FROM					
	Holsheim.	Vissembourg.	Frankfort.	Cologne.	Sufflenheim.	Kalbsheim.
Silex	0.430	0.687	0.500	0.670	0.724	0.442
Sand, Quartzose, mixed	0.365	0.015				0.010
Alumine	0.095	0.182	0.327	0.240	0.118	0.112
Magnesia	0.014	0.006	0.015	0.012	0.020	0.020
Oxide of iron	0.030	0.016	trace	0.012	0.048	0.048
Lime						0.164
Water*	0.056	0.092	0.160	0.066	0.078	0.202
	0.990	0.998	1.002	1.000	0.988	0.998

* The number 0.202 comprises carbonic acid and water. Au.

I used in my experiments two kinds of sand. That which I have designated as white sand is silecious, and is used in making crystals at the manufactory of Saint Louis in the Vosges: it is fine, and very white. The sand which has no particular designation given it in the tables, and which I have sometimes called ordinary sand to distinguish it from the white sand, is that which is commonly used at the works in Strasburg: it is obtained from a plain to the south-east of the town, between the Rhine and the Ill. This sand is rather fine; small gravel is mixed with it, and it is, a very little, earthy; it is coloured quite red by the oxide of iron. This is, for the greater part, granitic, mixed with a little silex.

The above experiments, with iron and oxide of manganese, confirm the opinion now for some time entertained, that these two metals do not confer any hydraulic property on lime. These results were published in 1822, as well as those concerning silex. At the same period, Mr. Berthier published in Vol. VII of the *Annales des Mines*, several interesting experiments, which I shall state. This Engineer, also, having ascertained that iron, and oxide of manganese, imparted no hydraulic quality to lime, says—

"Various mixtures, of chalk and ordinary sand, having been burned in a lime kiln, we obtained only meagre lime not hydraulic, and it was ascertained that only the twentieth part of the sand had been attached and rendered soluble by the alkalis.

"By substituting, for ordinary sand, the sand of Aumont, prepared for the manufacture of the Sévres porcelain, that is to say, reduced to powder by grinding, the combination is better; but all the silecious matter was not yet attached, and there remained about one third, which could not be dissolved in alkalis. We afterwards calcined for about one hour, in a platina crucible, at the temperature of about fifty pyrometric degrees, a mixture of ten grammes of chalk and one fifth of gelatinous silex, that is to say, silex separated by acids from its solution in alkalis. The matter slaked with considerable heat, and swelled a little: it formed a consistent paste with water, and, after two months immersion, the paste had acquired strength enough to resist the impression of the finger.

"We calcined, in a platina crucible, ten grammes of chalk with quantities of hydrate of alumine corresponding to 1.942 grammes in one experiment; and 2.36 grammes, in the other: the two mixtures slaked promptly with a strong heat, and swelled considerably. Soft pastes were made of these and put under water: but at the end of two months they had not acquired the least consistence.

"Ten grammes of the magnesian carbonate (of lime) of Paris, and two grammes of gelatinous silex, gave a lime which slaked with feeble heat and little swelling, and after a very short immersion became harder than the best artificial hydraulic lime. This lime may be stated to be composed of lime, 0.560; magnesia, 0.166; silex, 0.274.

"An essay made on a large scale with four parts of chalk, and one part of the kaolin of Limoges, induces the belief that it will be advantageous to have the quantity of alumine equal to the quantity of silex. This lime must be considered as composed of lime, 0.745; alumine, 0.125 ;and silex, 0.130; it acquired, very soon after its immersion, a consistence stronger than that of the artificial lime prepared with four parts of chalk, and one part of the Passy clay.

"A lime prepared, on a large scale, with four parts of chalk and one part of yellow ochre (in volume,) and which should contain lime, 0.745;

alumine, 0.055; oxide of iron, 0.070; silex, 0.130, took but a feeble consistence, even after being immersed a long time. We cannot help attributing the bad quality of this lime to the oxide of iron, because with a quantity of clay equal to the quantity of ochre used, we always obtain a lime eminently hydraulic.

"We calcined, in a platina crucible, ten grammes of chalk, 2.50 of carbonate of magnesia, and 2.50 of gelatinous silex. The matter was purplish; it slaked, giving out heat. A paste was made of it which was placed in water: at the end of two months it had acquired no consistence, while the chalk and silex alone, would have given a lime hardening promptly."

Mr. Berthier thinks that no mixture of which silex does not form a part, will acquire hydraulic properties: that appears probable; but, to be able to affirm it positively, it would be necessary to heat mixtures of lime and alumine or magnesia, which had not already sustained a first calcination. Mr. Berthier does not say whether the alumine which he mixed with his lime was derived from the decomposition of alum, or was natural alumine. In the first case, it is probable it had already sustained a previous calcination, and if so, it is not surprising that no result was obtained.

Mr. Berthier concludes from the experiment which he made with four parts of chalk and one part of ochre, that the oxide of iron was injurious. The oxide of iron enters, however, only in the small quantity of 0.070; and according to the analysis he gives of the English stone, (which furnishes Parker's cement?) it exists therein to the extent of 0.086, and, in the Boulogne pebbles, to the extent of 0.150; this last quantity being more than double, and these two limes being very hydraulic, notwithstanding.

We see that the experiments of Mr. Berthier and mine accord, as to hardening. The learned Professor did not submit his specimens to pressure, to ascertain the tenacity; it is probable he would have had results analogous to mine. I made no experiments by combining, as Mr. Berthier did, carbonate of magnesia with silex, which produced a lime hardening very promptly. It is desirable to repeat this experiment; afterwards submitting the mortars to pressure, to learn their tenacity.

A part of my preceding experiments having been printed in 1824 in a small pamphlet which was sent to the public works, and was inserted in the seventh number of the *Mémorial de l'Officier du Génie*, Mr. Vicat published, in the *Bulletin des Sciences*, in 1825, and in the *Annales des Mines*, Vol. X, 3d livraison, page 501, some observations on the results that I had presented. I propose answering the observations of Mr. Vicat, as the more suitable occasions present themselves. This Engineer commences his remarks in the following manner: "In proscribing hydraulic limes, Mr. Treussart rests upon experiments which do not appear to be conclusive: in the mixtures he made, sometimes he did not employ the clay in suitable proportions, sometimes he used a clay too aluminous, or charged with too great a quantity of oxide of iron, and sometimes he used quartz pounded very fine, as if the degree of fineness obtained by simple trituration could be compared to that of the silex contained in clay."

I do not know what induced Mr. Vicat to say that I proscribed hydraulic lime; the following are the expressions which conclude the pamphlet in question. "We see, by what precedes, that the principal idea of the author (Mr. Treussart) is, that there would be much more advantage in making, *directly*, mortar with fat lime, and trass, or factitious puzzalona, than in seeking first to make hydraulic lime, and afterward to compose mortars of this lime and common sand." This is very different from what Mr. Vicat makes me say. It is now more than twenty-five years since I learned to appreciate the good effects of hydraulic limes, whether for constructions in the air or in water: I have employed them wherever I could procure them: during the nine years that I was at Strasburg, there were built, with hydraulic lime, in the works in water and in air, more than a million of masonry: I have, then, used this lime for a long time, and I think few Engineers have used more; it would be strange, therefore, in me to proscribe it. I was bound to show the precautions that should be taken in using it, so as not to risk making very bad mortar out of very good lime; I was bound to say what my experience had led me to think, namely, that, in places where good natural hydraulic lime could not be found, it is preferable, as regards the quality of the mortar, and as regards economy, to make hydraulic mortar, directly, by using fat lime and factitious trass, in lieu of attempting to make artificial hydraulic lime with which to compose mortars by the addition of common sand: the sequel of my experiments will leave no doubt in this respect. As to the reproach that Mr. Vicat brings, of my having used clays which were not suitable; I have to answer that we were obliged to use those afforded by the locality. I made various essays with argillaceous earths of the environs of Strasburg, and I could do no better. Mr. Vicat says, page 7 of his memoir, that his method consists "in kneading together lime and a certain quantity of gray or brown clay, or simply brick earth, facilitating the operation by the aid of a little water." He gives no other indication as to the nature of the clays that should be used. The experiments that I have given in Table No. IX, prove that it is not indifferent what kinds of clay are used, for the results are not, by a great deal, the same for all. We do not yet know the proportions that a clay should have, to afford the best results. I do not comprehend the reproach connected with having tried silecious sand pounded very fine. I know very well that we cannot compare our mechanical process with the processes of nature; but having seen that Mr. Descotils and Mr. Vitalis gave two different analyses of the Senonches lime, I wished to ascertain whether very fine silecious sand would give a hydraulic result: for if so, I should conclude, however feeble the result might have been, that limestones containing a mixture of silex only, might give good hydraulic limes. The objection that Mr. Vicat makes to me did not prevent his employing, in mixing clay with lime, a mechanical process of which the result cannot certainly be compared to mixtures existing in natural hydraulic limestones; still he was right in trying the mixture, since it succeeds with certain clays.

When I saw that the clays of the environs of Strasburg gave results very inferior to those which I obtained with mortar made of common lime and factitious puzzalona, I requested the *Chef de Battaillon*, Augoyat, of the Engineers, residing in Paris to send me a little of the hydraulic lime made at Paris by Mr. Saint Leger, according to the process of Mr. Vicat. He sent me some, directly as it came from the kiln; a part was in masses, the rest had been broken into small pieces and put into a bottle, which was then well sealed. I made, with the lime as soon as it arrived at Strasburg, experiments like those I had made with natural and artificial hydraulic lime. The following table contains the results of these new experiments.

TABLE No. X.

No. of the mortar.	Composition of mortars.			Made immediately.		At the end of 1 month.		At the end of 2 months.	
				H.	W.	H.	W.	H.	W.
				d's	lbs.	d's	lbs.	d's	lbs.
1	Paris hydraulic lime slaked to powder and measured in powder Sand	1 2	3	12	187	15	88		
2	Same mortar			14	220				
3	Same mortar			16	246				
4	Same lime slaked in the air and measured in powder Sand	1 2	3			20	55		
5	Same lime, measured in paste Sand	1 2	3	15	253	25	99	30	88
6	Same lime Sand	1 2¼	3¼	18	237				
7	Same lime Sand	1 2½	3½	20	209				
8	Same lime, measured the same Sand	1 2	3	15	242				
9	Same lime Sand	1 2¼	3¼	18	220				
10	Same lime Sand	1 2½	3½	18	187	24	88		
11	Lime of No. 5 slaked in air and measured in paste Sand	1 2	3			30	88		
12	Lime of No. 5 slaked to powder and measured in paste Sand Trass	1 1 1	3					6	297

Observations on the experiments of Table No. X.

All the above mortars were made in the same way as the mortars of the preceding tables, that is to say, except for the mortars made of air-slaked lime; the lime was slaked to powder with one-fifth of its volume of water, whether made immediately or preserved for the periods indicated in the table. The mortars being made, they were put into boxes, and left in the air during twelve hours, like the others; after which they were put in water, and at the end of a year submitted to the proof of weights.

The first four numbers were made in the month of April 1823. No. 1 is proportioned with one part of lime slaked to powder, and two parts of sand: of this I made a mortar immediately after the slaking of the lime, and put it in water twelve hours afterwards: this mortar hardened in twelve days, and broke under a weight of 187 lbs. I left the rest of the lime in powder, in the air, and at the end of a month I made a mortar of it in the same way the first mortar was made. We see that the hardening required fifteen days, and that it would support only 88 lbs. No. 2 is a repetition of No. 1, being made of another piece of limestone: this piece appears to have been better than the first, since, of the two mortars made in the same way, the second result is sensibly the best.

The first two numbers were made of lime sent in masses without being defended from the contact of the air. No. 3 was proportioned in the same way, but it was made of broken lime, sent in a well sealed bottle; this number gave a better result than the preceding, which I attribute to the lime being kept from contact of the air. Six or eight days may have elapsed from the time of taking the lime from the kiln, to the time of making the experiment. No. 4 was made of the piece of limestone of which No. 1 had been made; it was left to slake in the air, and at the end of a month I made a mortar with it, taking one part of this lime in powder and two parts of sand. We see that the hardening required 20 days, and that the mortar broke under the light weight of 55 pounds. The lime No. 1 which had been slaked to powder with water, gave a better result, after the same lapse of time.

The first four numbers were proportioned with one part of lime slaked to powder, and two parts of sand; a short time after having made these, however, I became convinced that hydraulic limes would not bear as much sand as had been commonly thought, as I have before stated. I, in consequence, requested Mr. Augoyat to send me some more artificial hydraulic lime from the same manufactory of Mr. Saint-Leger. This lime was sent to me like the first, partly in masses *(en pierre)* in a box, and partly broken up and put in bottles well corked and sealed. This was received in the month of May 1824, and it was with this lime that I made all the experiments of the above table, which follow No. 4, taking, for the proportion, one part of lime measured in paste, against the quantities of sand stated in the table. Mortar No. 5 being made with lime measured in paste, contains much less sand than mortar No. 1, in which the lime was measured in powder. Nos. 5, 6, and 7 were made of the lime contained in the bottles. The object of these three experiments was to ascertain the quantity of sand this lime would bear. We see that the mortar made of one part of lime in paste to two of sand, gave the best result. As I made no experiments with a less proportion of sand, I am not able to say whether this is the best proportion. I anticipated that the best result would be found to lie between two parts and two parts and a half, of sand, because I partook of the common error, that it was better to give too much, than too little sand. In making No. 5, I had slaked a certain quantity of lime to dry powder, by one-fifth of its volume of water, and I left a portion thereof exposed to the air, as I did with the natural hydraulic lime of the environs of Strasburg; at the end of one and two months, I made new mortars proportioned like those made immediately after slaking. We see that at the end of one month, this mortar had already lost nearly two-thirds of its resistance, and at the end of two months it had lost still more; we see, also, that the hardening was much slower.

Nos. 8, 9, and 10 are repetitions of Nos. 5, 6, and 7, with this difference only, that the former were made of lime sent to me in a box, while Nos. 5, 6 and 7 were made of lime sent in bottles. The best result with these is, again, that in which there is one of lime to two of sand. Of the mortars of these six experiments, if those be compared which have the same proportions, it will be seen that the mortars made of lime secured in bottles were always better than those made of lime which had not been perfectly shut from the air, notwithstanding that only about six days had elapsed from the taking the lime from the kiln to the making it into mortar. I have already remarked that No. 3, made of lime enclosed in a bottle, gave a better result than No. 2, of which the lime had been exposed to the air. We see therefore that this artificial hydraulic lime, loses part of its hydraulic properties, promptly, by exposure to the air, as well as the natural hydraulic limes of *Alsace.*

No. 11 is an experiment made with the same lime as No. 5; but instead of slaking to powder with one-fifth of its volume of water, it was left to slake in the air for one month. At the end of this time it was reduced to powder, and I made it into paste by adding a little water, so that the proportions could be adjusted as in the other trials. The table shows that the hardening required 30 days, and that the mortar broke under a weight of 88 lbs., while mortar No. 5, composed of the same piece of lime, but made immediately, hardened in fifteen days, and sustained a weight of 253 lbs. before breaking. The comparison of No. 5 and No. 10 shows that lime which has been slaked to powder, has lost rather less of its quality at the end of a month, than that left to slake in the air during the same period. I will add, that when they sent me, in 1823, the lime of which the first three experiments were made, they also sent a little lime which had fallen spontaneously to powder. I left it in this state for two months; and I presume it had then been about three months exposed to the air: I made a mortar of this lime, taking one part of lime in powder to two parts of sand: the mortar which resulted was not able, after a years' immersion in water, to support the weight of the scale pan, which was 22 lbs.

No. 12, containing trass, was made with the same lime as No. 5, slaked to dry powder for two months. We see that No. 5, which was made of one part of this lime measured in paste, and two parts of sand, required thirty days to harden, and broke under the light weight of 88 lbs., while No. 12 hardened in the space of six days and supported 297 pounds before breaking. We see, therefore, that trass has the same influence on artificial hydraulic limes, as on natural hydraulic limes, and that it restores to them properties which they had lost by too long exposure to the air. I will here state that I was, last year, at the country seat of a Paris banker, who, having a spring of water, had caused a pond to be made in his garden. The spring being small, they had been obliged to cover the bottom of the pond with concrete, and to form the sides with a wall, all made of the artificial hydraulic lime of Mr. Saint Leger. The basin had been finished about a year, but a great deal of the water filtered through it: I examined the mortar and found it had a very feeble consistence: I questioned the architect as to the manner he had treated the lime, and he told me that he had caused a considerable quantity to be brought at once, and deposited in a spot sheltered from the rain, that sometimes he used it immediately, as it arrived, and sometimes, after it had fallen to powder. This explanation showed me that the evil proceeded from having used the lime that had been too long from the kiln, and which had already lost a great part of its hydraulic property.

Mr. Vicat appears not to have observed the bad results obtained with hydraulic limes which have been exposed some time in the air, whether slaked to powder with water, or slaked spontaneously: he even appears to incline to the process of air-slaking, for, after having given the three modes of slaking, he says, page 20: "such are the modes of slaking: the first is generally used; the second has hardly been employed except by way of experiment at certain works; the third is proscribed, and represented in all treatises on construction, as depriving the lime of all energy, to such a degree that all lime fallen to powder by air-slaking is considered as lost. We do not now allude to the processes of Rondelet, Fleuret, and others, because they do not differ enough from those described to be separated from them.

We shall see, further on, relative to spontaneous slaking, how necessary it is to be mistrustful of condemnatory assertions, springing out of false observations, and accredited by authors who know how to doubt of nothing, and who repeat, without examination, the errors of others."

At page 25, Mr. Vicat says, again, "the three modes of slaking arranged in the order of superiority, relative to the resistance, and the hardness they communicate to the hydrates of common fat lime, are: 1st. Ordinary slaking; 2nd. spontaneous slaking; 3d. slaking by immersion. The mean relative resistances, in the three cases, are the numbers 2490, 1707, 450, and the hardness as 0.^{d}1696, 0.^{d}0850, 0.^{d}0713.

"The order changes for hydraulic lime, and becomes: 1st, ordinary slaking; 2d, slaking by immersion; 3d, spontaneous slakings. The mean relative resistances are as the numbers 864, 392, 245, and the hardness as 0.^{d}0488, 0.^{d}0446 C.^{d}0358."

The above experiments of Mr. Vicat were made with hydrate of lime, that it to say, with limes reduced to paste with water. We see that ordinary slaking gave better results than spontaneous slaking; with both fat lime and hydraulic lime. In his table No. XVIII., which contains eight mortars made with diverse hydraulic limes, and sand; the first five gave greater resistances when the lime was slaked in the ordinary way than when slaked spontaneously. The mortars made of fat lime and sand, as exhibited in tables Nos. XIX., XX.. and XXI., gave, it is true, better results when these limes were slaked spontaneously: but with hydraulic limes he, himself, obtained results very different, since the mortar No. 5 of table No. XVIII. gave a resistance represented by 4102 when the lime had been slaked by the ordinary process, while it was only 3082, when the lime had slaked spontaneously. The experiments of Mr. Vicat are, therefore, far from causing us to reject the old saying of masons as to the bad results obtained from air-slaked lime; and we do not perceive how he could regard this saying as a condemnatory assertion growing out of false observations. This old saying appears to me to be well founded as to the greater number of hydraulic limes. Mr. Vicat, in giving his experiments, had not said for how long a time he left the limes which he slaked by the second and third processes, in the air. If he had made comparative experiments, he would have had very variable results, according as the hydraulic limes used had been, for a greater or less time, exposed to the influence of the air.

All that precedes proves that hydraulic limes, whether natural or artificial, lose much of their hydraulic property if not used soon after they are calcined. As an exception, a single piece of lime, of which the experiments in table No. VI. were made, gave to the mortar, after fifteen days slaking, a better result than to the mortar made immediately; but we see that the resistances of the mortars made at periods more remote from the slaking, diminished rapidly: we may therefore say, in relation to all the hydraulic limes that I have treated, that it is necessary to make the mortars soon after they come from the kiln; for I assured myself that the same deterioration took place with the limes of Bouxviller, Oberbronn, Verdt, &c., although I did not make as detailed experiments with them as with the Obernai lime. All, in fine, induces me to believe that the greater part of hydraulic limes are thus affected. It is consequently very important, before using them, to see if they do not soon lose, by exposure to the air, a great part of their hydraulic quality.*

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* We see, by the above, how many precautions should be taken with hydraulic lime. Parker's cement which has been burned and pulverized, is brought from England: this

The artificial hydraulic lime, made by Mr. Saint-Leger at Meudon, to supply the wants of the Capital, is made of chalk which is mixed with $\frac{2}{10}$ of clay brought from Vanvres, near Paris. This clay contains the following substances: silex, 0.630; alumine, 0.282; oxide of iron, 0.068. A cubic metre of this lime (35.34 cubic feet) costs at Paris about $13,50. If it be examined minutely, there will be seen small portions of clay that have not been well mixed. This is an inevitable result, because we cannot, by a mechanical operation, obtain a mixture as intimate as that existing in natural hydraulic limes. The results which I obtained with this lime are a little superior to those that I got from the Holsheim clay, of which the resistances varied from 187 to 215, while with the hydraulic lime of Paris, I obtained from 187 to 253. This might be owing to their having sent me choice pieces of lime. I made, besides, too few experiments with the Holsheim clay to be able to affirm that it is really inferior to that of the environs of Paris for making artificial hydraulic lime.

Captain Petitot of the Engineers sent me from *Vitry-le-Francais* a little hydraulic lime of the environs of that place of which he had made trial, obtaining, as he informed me, good results. This lime comes from chalk; it is very white; water being sprinkled on it, as it comes from the kiln, about five minutes elapses before slaking begins. One part of quick lime gave 1.30 of paste. I do not know the degree of resistance that mortars made of this lime would give; but I made the following experiment with it: I took a portion which was sent to me in a well stopped vessel, as soon as it was withdrawn from the kiln; I made a paste of this with water; I left it in the air for one hour, and then plunged it in water: it took a good consistence in a very short time, and at the end of four days it was very hard. This lime, therefore, hardens more promptly than any of the hydraulic limes of Alsace and Metz, and than any of the artificial hydraulic limes that I have examined. The only exception is the Obernai lime, shown in table No. VI. The Boulogne pebble lime hardened sooner; but it is remarkable that the mortars of this lime which hardened soonest, presented a weak resistance; while those which were slower to indurate, gave a greater. The Vitry lime appeared to me to harden in the manner of good hydraulic lime, and to promise to furnish very good mortars. This lime costs in the neighborhood, only $5.50 per cubic metre (35.34 cub. feet,) while we see, above, that the Meudon lime costs about $13.50. It would therefore, be a good speculation to bring the hydraulic lime necessary for the Capital, from Vitry. The river Maine, which is navigable as far as Vitry, would give great facility to the transportation. As wood is scarce in the neighborhood of Vitry, as coal is not easy to be obtained, and as it is advantageous to use hydraulic limes soon after calcination, it would perhaps be best to bring the chalk, of which the lime is made, to Paris; and to burn it with coal, as it might be wanted—profiting of favourable seasons for collecting a stock of the chalk.

The experiments which I made with the Boulogne pebbles, induced me to think that the soda it contained, might have some agency. I made, therefore, various experiments, by reburning fat lime with a little clay, and adding some water containing soda or potash. I proceed to give the results.

lime may be good in England, and have lost a great part of its properties on arriving in France. It is necessary, therefore, to try, before using it.—*Author.*

Table No. XI.

No. of the mortar.	Composition of the mortars.		LIME			
			Burned as commonly.		Burned more highly.	
			H	W	H	W
			days	lbs	days	lbs
1	Fatlime, reburnt with 2-10 of pipe clay, and measured in paste Sand	1 2 } 3	8	132	15	187
2	Same lime, reburnt with 3-10 of pipe clay, 1-10 of solution of soda at 5°, and measured in paste Sand	1 2 } 3	8	176	20	209
3	Same lime reburnt with 2-10 of pipe clay, and 1-5 of solution of soda at 5°, and measured in paste Sand	1 2 } 3	8	220	15	242
4	Same lime, reburnt with 2-10 of pipe clay, and 1-4 of solution of soda at 5°, and measured in paste Sand	1 2 } 3	8	176	18	231
5	Same lime, reburnt with 2-10 of pipe clay, and 1-3 of solution of soda at 5°, and measured in paste Sand	1 2 } 3	9	154	18	220
6	Same lime reburnt with 2-10 of pipe clay, and 1-2 of solution of soda at 5°, and measured in paste Sand	1 2 } 3	10	121	18	198

Observations on the Experiments of Table No. XI.

To make the above experiments I took fat lime which I reduced to paste, with water: I then mixed it well, with $\frac{1}{10}$ of the Cologne pipe clay, of which the analysis has been given: this is lime No. 1. For No. 2, I added $\frac{1}{10}$ of the solution of soda of the strength of 5° of the *pèse acid.* The volume of solution of soda was taken in relation to that of the lime in paste, and with it I diluted the pipe clay before mixing it with the lime. In the succeeding numbers, I successively tempered the pipe clay with $\frac{1}{5}$, $\frac{1}{4}$, $\frac{1}{3}$ and $\frac{1}{2}$ of the solution of soda of the same strength. Each of these experiments was made in duplicate; one half of each mixture was placed in such a situation in the lime kiln, that it would receive only the mean degree of heat: the other half was exposed to a higher temperature. I satisfied myself of the degree of heat sustained, by placing unburnt bricks by the side of the crucible containing the mixtures. From the colour of these bricks I knew that the second set had been more highly calcined than the first. Immediately after the calcination, I made mortars with one part of lime in paste and two of sand. The mortars are given in the table in two series according to the two degrees of calcination. It will be noticed, at once, that the more highly calcined lime, gave the greater resistance, but the slower rate of hardening. No 1, containing pipe clay only, gave a resistance of but 132 lbs. for the lime the least burned, and 187 lbs. for that most burned, No. 2, which contained $\frac{1}{10}$ of solution of soda, gave a slight augmentation of resistance, and No. 3, which contained one-fifth of solution of soda, gave a greater resistance for both degrees of calcination. Beyond the proportion of one-fifth solution of soda, the weight supported went on decreasing; but up to the proportion of one-fifth the resistance was increased, by the addition of a little soda, about in ratio of 6 to 7. These experiments were made in the month of January.

Having made the above experiments with clay and soda, I made similar ones with potash: the results obtained are in the following table.

Table No. XII.

No. of the mortar.	Composition of the mortar.	No. of days required to harden in water.	Weight supported before breaking.
		days	lbs
1	Fat lime, reburnt with 2-10 of pipe clay and 1-10 of solution of Potash at 5°, measured in paste . . . 1 Common sand . . . 2 } 3	6	242
2	Same lime, reburnt with 2-10 of pipe clay and 1-5 of solution of Potash at 5°, measured in paste . . . 1 Sand . . . . 2 } 3	8	297
3	Same lime reburnt with 2-10 of pipe clay and 1-4 of solution of Potash at 5°, measured in paste . . . 1 Sand . . . . 2 } 3	12	242*
4	Same lime reburnt with 2-10 of pipe clay and 1-3 of solution of Potash at 5°, measured in paste . . . 1 Sand . . . . 2 } 3	15	407
5	Same lime, reburnt with 2-10 of pipe clay and 1-2 of solution of Potash at 5°, measured in paste . . . 1 Sand . . . . 2 } 3	18	275

Observations on the experiments of Table No. XII.

The experiments in the above table were made in the same way, and at the same time, as the experiments of table No. XI. These limes were burned by the side of those of table No. XI, which gave the best results. No. 1 of table No. XI, containing pipe clay alone, serving equally as a term of comparison for the above table. We see, in table No. XI, that the greatest resistance obtained by the mixture of lime and pipe clay only, was 187 lbs. The experiments of tables Nos. XI and XII differ only in substituting in the latter, potash for soda. We see that the results obtained with potash are superior to those obtained with soda. No. 3 of the above table being cracked, I am uncertain whether the greatest resistance belonged to No. 3 or No. 4. No. 5 shows that the proportion of one-half potash, diminished the resistance. I made a sixth experiment by putting one-half of potash at 10°: the mortar resulting broke under a load of 242 lbs. It is remarkable that the mortars which gave the greatest resistance were those which hardened the most slowly. Several of the preceding tables gave the same result; it was quite striking with the Boulogne pebbles of table No. VIII. It will there be seen that No. 1 which hardened in twelve hours, supported a weight of only 108 lbs., while No. 12, which supported 286 lbs., required 12 days to harden. An inspection of the preceding tables will show that, in general, the mortars which gave good results, took from 8 to 15 days to indurate, whether made of natural or artificial hydraulic lime. The experiments of table No. XII were made in the month of January. I also made essays with the soda and potash of commerce, which, as is known, are composed of sub-carbonates, sulphates and muriates of these substances: the results obtained were not so good.

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* This mortar was cracked on being cut, which diminished its strength. Au.

I made several experiments with lime and solutions of potash and soda without other admixture. I operated in this way: I took several pieces of fat lime as they came from the kiln, and slaked them to dry powders thus,—on the first piece I sprinkled the fourth of its volume of solution of soda standing at 5° of the *pèse-acid*; on the second, the same quantity of solution of the sub-carbonate of soda of commerce, of the same strength; on the third, the same quantity of an equally strong solution of potash: on the fourth a like quantity of solution of the same strength of the sub-carbonate of potash of commerce; and on the fifth an equal quantity of solution of muriate of soda, also standing at 5°. I left these several powders in the air for one month; I then added the quantity of water requisite to produce a paste, and had them reburnt in a lime kiln. As they were drawn from the kiln I slaked them with a little water, and left them in the air, for some days: I afterwards made mortars of them, by mixing them with sand, and having allowed them to harden in the air for twelve hours, I placed them in water: the two first mortars hardened in two days; the third, the fourth and the fifth hardened in a day and a half. At the end of a year, I wished to break these mortars: and I was much surprised to find that, notwithstanding they had hardened so promptly, they had become entirely soft. As the boxes containing these mortars were covered with others, I had not been able to observe them at the period to their beginning to lose hardness.

It follows from experiments made up to this time, that it is clay,—that is to say, a mixture of alumine and silex, united with the lime, that gives good hydraulic lime by calcination. (An exception exists in the lime of Senonches, which is said to be very hydraulic, and which appears to owe this property to silex alone.) We do not as yet know the proportions in which silex and alumine ought to exist, to give the best results. Hydraulic limes are improved by adding to the mixture of clay and lime, a little soda: the result is still better if potash be used instead of soda. We have seen that the Boulogne pebbles gave a lime which hardened very quickly, but which would not bear mixture with any other substance, in making mortar—this being the only lime which gave this result. I before observed that it appeared to me these pebbles contained a little soda: but we have just seen that limes which I caused to be reburned with a little soda or potash gave good mortars when mixed with a considerable quantity of sand: it seems, therefore, as I have remarked, that these pebbles contain some substance which escaped analysis, or else that some of the substances known to be present are modified by the sea-water.

In comparing artificial hydraulic limes, I have been led to remark that better results were obtained when, in lieu of making the mixture of clay, with lime just from the kiln, I first slaked the lime with one fifth of its bulk of water, and left it, in this state, in the air for a month or two. I do not attribute the better effect solely to a more minute division of the lime: but I think that the slaked lime exposed to the air, absorbing much oxygen, as I have shown in page 318, is capable, in this state of oxidation, of combining better with the clay which is mixed with it. I proposed making experiments to ascertain what might be the advantages of this process, and also during what time it would be proper to have exposed to the air, lime that had been slaked to powder, or lime designed to be left to spontaneous slaking; but I was obliged to quit Strasburg, before having it in my power to make these experiments.

ART. V.—*On Hydraulic Mortars made of fat lime and trass, or fat lime puzzalona.*

Trass is a substance obtained from the village of Brohl, near Andernach, on the Rhine; this village is situated at the foot of an extinct volcano. Trass is of a grayish colour, much resembling gray clay which has been calcined. I have seen several pieces of trass which were covered with lava. This last substance differs much from trass: the separation is distinct: the lava which covers the trass is of a blackish colour, and its surface is full of asperities and cavities, showing that it has sustained a very high heat, and very rapid cooling: trass seems to have been exposed to a much lower heat.

Puzzalona is likewise a burned clay—deriving its name from the village of Puzzoles, at the foot of Vesuvius; it is found at or near the surface. According to Mr. Sganzin, there are a great many varieties of puzzalona: it is found white, black, yellow, gray, brown, red, and violet.

It is not known whether trass and puzzalona result from banks of clay which have been highly heated by incumbent lava, or whether they have been thrown out of volcanos during irruptions; it is possible that both suppositions may be true; an examination on the spot can alone remove uncertainty in this respect.

Puzzalona is, according to Mr. Sganzin, also obtained near Rome: this is of a red-brown, and contains brilliant particles having a metallic lustre.

The Dutch have a great trade in Trass. They get out this substance in masses, and reduce it to a very fine powder, by means of wind mills. Much has been sent to France, to the North, and to England; but it seems that the commerce has diminished a little. Some authors call this substance the Terras of Holland.

Arches, in several of the gothic churches, in Belgium were built of pieces of the Andernach Trass: as was the case in several edifices on the Rhine. The great lock of Slikens in Belgium, was rebuilt with pieces of the arch of a demolished church; and by pulverizing the same material, a very good trass was obtained.

On the shores of the Mediterranean, much puzzalona, furnished from the environs of Rome and Naples, is used. The good effects of this substance were known to the Romans, for Vitruvius, Book II., Chapter VI., has the following passage. "There is a kind of powder to which nature has given an admirable quality: it is found near Baie, and in the grounds around Mount Vesuvius. This powder mixed with lime and stones, not only renders common masonry very firm, but it even unites and hardens in a remarkable manner, masonry at the bottom of the sea."

I had occasion to appreciate the good effects of trass in the great works of Vesel. It was brought in powder, and as it was transported in batteaux descending the Rhine, it was not dear. Before becoming acquainted with the excellent qualities of the Obernai hydraulic lime, I caused some of it to be brought to Strasburg, and the first hydraulic works that I constructed at Strasburg were made of mortar composed of fat lime, sand, and trass. But the trass cost much more at Strasburg than at Vesel, on account of the export duties that the Prussians levied on it, and because of the difficulty of ascending the Rhine. A cubic metre (35.34 cub. feet) cost 120 francs (about $23.00) at Strasburg, which raised the cost of the hydraulic mortar enormously. I had begun some researches with a view to make artificial trass, when I became aware of the hydraulic properties of the Obernai lime. This lime was afterwards used without trass in the foundations of revetments, in the construction of bridges, and in all masonry exposed to the air; but in the foundation of locks and dams and in the construction of caps for under-ground casemates, a little trass was always mixed in the mortar, with the lime and sand. I continued my experiments on the forming of artificial trass, less for our particular advantage at Strasburg, therefore, than for those places which do not afford good natural hydraulic limes. I succeeded, in the end, as will be seen in the following Article, in making artificial trass equal in quality to natural trass or puzzalona. The cost of a cubic metre (35.34 cub. feet) was about 30 francs (about $6.00.) I caused the puzzalona to be brought from Naples, it was red, and much resembled brick or tile dust.

An analysis of trass and puzzalona, made many years ago, may be found in the *Précis du cours de construction* of Mr. Sganzin of the Polytechnic school. Mr. Berthier asked me for samples of the trass and puzzalona which I used at Strasburg, and made an analysis of them. Both analyses are given below.

	Old analysis.		Mr. Berthier's analysis.	
	Trass.	Puzzalona	Trass.	Puzzalona
Silex	0.570	0.350	0.570	0.445
Alumine	0.230	0.400	0.160	0.150
Lime	0.065	0.050	0.026	0.088
Magnesia			0.010	0.047
Oxide of iron*	0.085	0.200	0.050	0.120
Potash			0.070	0.014
Soda			0.010	0.040
Water			0.096	0.092
	0.950	1.000	0.992	0.996

We see that the new analysis of trass and puzzalona by Mr. Berthier differs in several points from the preceding analysis. According to that of Mr. Berthier, trass and puzzalona contain potash and soda which were not before noticed in those substances. According to the old analysis, puzzalona contains much more alumine than trass, but according to Mr. Berthier, about the same. The proportion of alumine to silex is greater, for both substances, in the old, than in the new analysis. I will add that Mr. Berthier discovered that the trass and puzzalona which I sent him attracted the magnetic needle.

We ought not to be surprised that these analyses differ sensibly, because it has been known for a long time that different puzzalonas give very different results, and it is the same with trass. We can conceive that these substances may vary considerably, because the several strata of clay in the same bank often vary sensibly. I proceed to give different experiments made with fat lime, puzzalona, and trass.

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* In Mr. Berthier's analysis, there was in the Puzzalona, oxide of iron and titanium 0.120.—Author.

Table No. XIII.

No. of the mortars	Composition of the mortars.			No. of days which they took to harden in wat'r	Weight which they supported before breaking.
				days.	pounds.
1	Fat lime of Strasburg slaked to powder and measured in powder	1	3	5	411
	Sand	1			
	Trass	1			
2	Lime the same, do.	1	3	4	330
	Trass	2			
3	Lime the same, do.	1	3	4	499
	Sand	1			
	Puzzalona	1			
4	Lime the same	1	3	3	444
	Puzzalona	2			
5	Fat lime from Vasselone, do.	1	3	5	449
	Sand	1			
	Trass	1			
6	Lime the same, do	1	3	4	385
	Trass	2			
7	Fat lime from Brunstat, do.	1	3	5	510
	Sand	1			
	Trass	1			
8	Lime the same, do.	1	3	4	535
	Trass	2			
9	Lime from white marble, do.	1	3	5	308
	Sand	1			
	Trass	1			
10	Lime the same, do.	1	3	4	407
	Trass	2			
11	Same lime, do.	1	3	4	396
	Sand	1			
	Puzzalona	1			
12	Lime the same, do.	1	3	3	367
	Puzzalona	2			
13	Strasburg fat lime measured in paste	1	3	4	495
	Puzzalona	2			
14	Lime the same	1	3½	5	550
	Puzzalona	2½			
15	Same lime slaked to powder and measured in powder	1	3	3 to 16	231 to 580
	Trass	2			

Observations on the experiments of Table No. XIII.

The lime of the first twelve numbers was measured in powder and mixed with the substances indicated in the table. It will be seen that I used various fat limes of the environs of Strasburg. I satisfied myself that these limes had no hydraulic property. The first four numbers were designed to ascertain whether puzzalona is preferable to trass. The puzzalona gave me rather the best results—the lime being the same in all. The experiments 9, 10, 11, and 12 confirm the first four. We see that I had a greater resistance by mixing sand with the lime and puzzalona, or lime and trass, than when there was no sand. The limes of Nos. 13 and 14 were measured in paste, and we see that I obtained a better result by augmenting the proportion of puzzalona.

No. 15 contains the results of many trials made with 1 part of fat lime measured in powder and 2 parts of trass. The time of hardening varied from 3 to 16 days, and the resistance from 231 to 580 lbs. But I had few mortars giving a result so feeble as 231 lbs., or so strong as 580 lbs. We might say that the mean resistance with mortars composed of fat lime, sand and trass, was between 352 and 374 pounds. The mortar which gave 580 pounds contained lime and trass only. If, with the same substances, I had composed a mortar of lime, sand, and trass, I should have had, probably, a still better result. I found fewer variations with puzzalona than with trass, probably because, having but a small quantity of the former, my experiments were few. We shall, however, see in table No. XX., that a mortar like No. 13 of the above table, broke under a weight of 286 lbs. I am uncertain whether this weak result was due to particular circumstances or to the quality of the puzzalona. It appears to me, in general, that there was no great difference between the trass and the puzzalona, that I used.

In comparing the results of table XIII., with those of the preceding tables in which mortars were made of natural or artificial hydraulic lime and sand only, we see that these last hardened much more slowly, and gave much less resistance, than mortars made of fat lime, sand, and trass or puzzalona. Mr. Gauthey was therefore in error, as I noticed page 235, when he stated that mortars made of fat lime and puzzalona did not take consistence in water and remained pulverulent.

In the observations made by Mr. Vicat on the pamphlet which I published in 1824, and of which I have already spoken, this Engineer remarks as follows: "The greatest resistance that Mr. Treussart found for trass mortars immersed for one year, is 17 $\frac{6}{10}$ lbs. for four-tenths of an inch square, and I have made mortars with lime and sand, which, after a year's exposure on a roof to all the inclemencies of the seasons gave a resistance of 31 lbs. and even of 40 lbs." The results that Mr. Vicat and I obtained are very different. These differences can only be attributed to our having used means of measuring the resistance, or rather the tenacity, of mortars, which do not admit of comparison. I will observe, however, that the greatest resistance I found was 579 lbs., for mortar made of artificial trass, and that the section being four square inches, there results 23.1 lbs. for four-tenths of an inch square instead of 17 $\frac{6}{10}$ lbs., as Mr. Vicat supposes. He does not say what were the dimensions of the mortars he submitted to proof in order to compare his results with mine; and as a year had not elapsed from the publication of my pamphlet to the appearance of his observations, I conclude that he did not use the same method in breaking the mortars of which he speaks and which he says had been made a year, that I did. Of twenty-five tables that Mr. Vicat's memoir contains, the weights supported by mortars, are given only in three instances—to be found in table No. III. All the other results of the tables are resistances obtained by calculation from the penetration of a rod subjected to an impulsive force, and deduced from the few experiments of table No. III. This table indicates that the mortars had 1 inch 6-10 of base, by 1 inch in height, and that the weights were applied at the extremity of a lever of only 1.$\frac{2}{10}$ long. But the mortars which I submitted to fracture, were two inches square, they rested on two points of support, distant four inches from each other; the weights were applied to the middle of the mortar: thus the dimensions of the mortars and the arms of the levers to which the weights were applied, were entirely different, in the two methods. We, cannot therefore, compare the weights supported by four-tenth of an inch square, when the methods were so different. I will add to what I said page 305, that if Nos. 26 and 33 of my table No. III. be submitted to the proof of the rod of Mr. Vicat, it is possible that the rod would penetrate less into No. 33, which contains three parts of sand, than into No. 26 which contains none. Whence we might assign to No. 33 a higher resistance than to No. 26, whereas its actual resistance is three times less. If sand (alone) be compressed in a metallic tube, and then submitted to the rod of Mr. Vicat, which is terminated by a flat portion, it will penetrate but little into the sand; and the false consequence would appear that the sand possessed tenacity. It is to be noticed that when the lime takes consistence, it fulfils, as regards the resistance of the sand to the shock of the rod, the function of an enclosing vessel. The means used by Mr. Vicat to determine the tenacity of mortars, appears therefore to be bad, and his results not of a nature to be brought into comparison with mine.

The experiments stated in the above table, and especially those comprised under No. 15, suggested anew to me an idea that I had already entertained regarding hydraulic limes: namely, that the differences in the results might not be attributable solely to diversity in the trass, but might result from using the lime sometimes immediately from the kiln, and sometimes after having been left for a period exposed to the air after having been slaked to powder. I consequently made experiments to know the influence exercised by the air on fat lime—these are stated in the following table.

Table No. XIV.

No. of series	Composition of the mortars.	Made immediately.	After 5 days.	After 10 days.	After 20 days.	After 1 month.	After 2 months.	After 3 months.	After 4 months.	After 5 months.	After 6 months.	After 7 months.
		lbs	lbs	lbs	lbs	ls	lbs	lbs	lbs	lbs	lbs	lbs
1	Fat lime slaked to powder and measured in powder 1 Sand . 1 Trass . 1 } 3	242 days (20)	231 d (18)	253 d (16)	275 d (12)	319 d (5)	396 d (4)	363 d (5)	374 d (6)	386 d (8)	407 d (10)	396 d (12)
2	Lime the same 1 Trass . 2 } 3	374 (16)	341 (11)	341 (8)	407 (6)	440 (4)	440 (3)	462 (3)	418 (4)	429 (6)	506 (8)	462 (10)
3	Fat lime air-slaked and measured in powder . 1 Sand . 1 Trass . 1 } 3					253 (9)	308 (6)	286 (6)	308 (7)	319 (9)	352 (12)	330 (12)
4	Lime the same 1 Trass . 2 } 3					319 (7)	363 (4)	385 (4)	352 (5)	341 (5)	385 (7)	352 (8)

Observations on the Experiments of Table No. XIV.

For making the above experiments I took fat lime as it came from the kiln, and slaked a part to dry powder with a quarter of its volume of water.

I then made two mortars, taking for the first, one part of this lime in powder, one of sand, and one part of trass, and for the second mortar the same quantity of lime and two parts of trass. These two mortars are in the first column, and will serve as terms of comparison for the rest. I then put in a vessel the remainder of the lime slaked to powder, and made mortars of it,of the series No. 1 and 2, at the periods indicated in the table. When I slaked this lime to powder I left another portion to slake in the air, and at the end of a month I commenced the series of experiments therewith, marked Nos. 3 and 4. I took care to use the same trass in all the mortars. I must remark that the figures in brackets express the number of days the mortars required to harden, and the figures over those in brackets, express the weights supported before breaking.

The series comprised under No. 1 was made of one part of lime in powder, to one part sand and one part trass. We see that the hardening of the mortars which were made soon after the slaking, was slow; at the end of a month it was much more prompt, and after three months, it again became slower. The numbers representing resistances present prominent anomalies; but we must look at the whole rather than at the particulars. The *whole* of this series shows that mortars made of lime slaked to powder, and exposed to the air for a month and more, gave better results than those made immediately, or soon after the slaking of the lime,

These experiments were commenced in the middle of April, 1823; the numbers, therefore, which express the most prompt hardening, correspond to the hottest months, and according to the observation of Mr. Vicat, mortars harden in the water more promptly in summer than in winter. The two last columns correspond to the months of October and November, and although the hardening had then, again, become slower, the resistance had not lessened. The experiments of table No. XIII. were made with lime which had remained slaked in powder about one month. The first twelve numbers were made in September and October and No. 13 and 14, in the month of January: I, however, got a prompt hardening. It appears that by exposing fat lime which has been slaked to powder, to the air for some time, the induration is favoured: but there is no doubt that heat contributes much thereto.

The series of experiments comprised under No. 2, were made of one part of lime in powder to two parts of trass. We see, here also, that the resistances that I obtained a short time after the slaking are inferior to those got one month after the slaking. The experiments of table No. XIII. often gave me with sand and trass, better results than with trass alone, while it is the contrary in the above table. I attribute this anomaly to the difference in the trass used.

The series comprised under No. 3 and 4, were made in the same manner as the two first series, with this difference that air-slaked lime was used. It was not before the expiration of a month that I had enough of this lime to commence the experiments. If we compare the third series with the first, and the fourth with the second, we shall see that the results are in general inferior with the air-slaked lime. I will add that all the mortars made of lime slaked to powder were very homogeneous, while those of air-slaked lime exhibited, in the interior, a multitude of white points which appeared to me to be particles of lime that had absorbed carbonic acid; they were very apparent when the mortars were broken. I think, therefore, that it is better to slake fat lime to a powder, than to air-slake it.

I ought to observe that before making the experiments oft able No. XIV.

I had made several of the same kind but less complete; and that I had often got contradictory results, that is to say, sometimes, I obtained a greater resistance when I had made mortar of lime just as it came from the kiln, than when I slaked it to powder and left it for sometime in the air. Sometimes, also, I got better results with air-slaked lime than with that slaked to powder; the differences were not great but they existed. On examining my notes, I saw that in making my first experiments, I changed the trass several times; this induced me to keep back these previous results, and the anomalies that I found therein determined me to make the experiments of the above table, in all of which the same trass was used.

These experiments should, no doubt, be repeated with several kinds of lime, to ascertain whether it will always be better to use fat lime as soon as it is burned, or sometime after having slaked it to dry powder. It is necessary to repeat the experiments both in summer and in winter. As to air-slaked lime, it appears to me that this process ought not to be adopted, because of the great quantity of white particles which I found in all the mortars of this kind that I made.

This manner of slaking fat lime is that which Mr. Vicat prefers: he, in *Annales de chimie*, Vol. XIX. page 22, publishing as follows: "The assertion of Mr. John relative to lime exposed to the air is in contradiction to recent facts, so presented, and so multiplied, that I am constrained to combat it. It was I who first announced that fat lime slaked spontaneously, and abandoned for one year to the action of the air, under cover and protected from winds, gives better results, than when employed immediately, according to the common method. This conclusion is founded on a hundred and fifty experiments, varied in several ways: it results, for example, that the force of ordinary mortar being, in the most favourable case of a series of experiments represented by 1506, that of mortar made of air-slaked lime gave, under the same circumstances 2293." Mr. Vicat acknowledges that the ideas, commonly received, on this point of the doctrine of mortars, are all in favour of Mr. John, and he says, because of this sentiment, almost general, he presumes Mr. John did not examine the matter. I will remark, touching this point, that Lieut. Col. Bergere of the Engineers, notices, in the last *devis instructive*, and in the account he has given of the work of Mr. Raucour, that several Engineers have thought, in times long gone by, that air-slaking gave better results than the other two processes, and that this mode has been in use, from time immemorial, in Spain, and in a part of Italy, Mr. Bergere says that he used it at Flushing; and that this method is recommended in a letter written in 1764 by Mr. Sienne, an Engineer officer, resident at Graveline. I do not however counsel the process of air-slaking in making hydraulic mortar with fat lime and trass, or other analogous substance; because the mortars made in this way contain a great many white specks which appear to be lime passed to the state of carbonate. We may conceive, in fact, that when lime is left to slake spontaneously, every successive small portion of lime remains for some time exposed to the contact of the air, as the lime falls off in successive layers: there ought, therefore, to be a considerable absorption of carbonic acid: whereas lime slaked to powder with water and formed into heaps, has the surface only exposed to the air.

Besides the experiments of the above table, I have made others of the same kind, by slaking fat lime into thick paste, and into thin paste—comparing these results with each other, and with those I got from the same lime slaked to powder.

These experiments are in the following table.

Table No. XV.

No. of series	Composition of the mortars.	Made immediately.	After 5 days.	After 15 days.	After 1 month.	After 2 months.	After 3 months.	After 4 months.	After 5 months.	After 6 months.	After 7 months.	After 8 months.	After 9 months.	After 10 months.	After 11 months.	After 12 months.
1	Fat lime slaked to thick paste 1 Sand . . . . . . 1¼ Trass . . . . . . 1¼ } 3½	lbs 319 (12)	lbs 341 (12)	lbs 352 (14)	lbs 374 (16)	lbs 341 (18)	lbs 363 (20)	lbs 341 (18)	lbs 385 (16)	lbs 352 (10)	lbs 341 (5)	lbs 319 (5)	lbs 341 (6)	lbs 330 (8)	lbs 341 (10)	lbs 363 (13)
2	Fat lime slaked to thin paste 1 Sand . . . . . . 1¼ Trass . . . . . . 1¼ } 3½									308 (10)	297 (5)	308 (6)	275 (6)	286 (8)	253 (10)	275 (13)
3	Fat lime slaked to powder and measured in paste 1 Sand . . . . . . 1¼ Trass . . . . . . 1¼ } 3½									396 (8)	352 (5)	341 (5)	352 (6)	363 (8)	341 (10)	352 (13)

Observations on the experiments of Table No. XV.

To make the experiments of the above table, I took fat lime as it came from the kiln, and divided it into three portions: one of these portions wa slaked to a thick paste and left in a vessel; another portion was slaked to a thin paste, and, as it thickened, I added a little water so as to keep it in the consistence of sirup; the third portion was slaked to a dry powder with one-fourth of its bulk of water, and put, like the others, in an open vessel. I, immediately, made the mortar in the first column, which may serve as a term of comparison. The others were made, at the periods expressed in the table, of lime slaked in the several modes mentioned. In proportioning the parts, I added, when I made the mortar, a little water to the lime which had been slaked to a thick paste and also to that which had been slaked to powder, so as to bring all to the condition of that slaked to thin paste. These experiments were begun in the month of November, 1823, that is to say seven months after the experiments of table No. XIV. In table No. XV, I have followed the same mode of expressing the time required to harden, as in table No. XIV. The mortars of three series were made of the same quantities of lime, sand and trass; and I took care, as in the preceding table, to use the same trass, and the same sand, for all.

In the first series of the table, the mortar made immediately gave a result rather weaker than those made afterwards: the hardening was more prompt in summer than in winter.

I left the lime of the second series for six months in a state of clear paste before using it. We see that the results were not so good as those obtained from the thick paste. There are, certainly, several anomalies in the results, but, as before remarked, we must look at the *whole.*

The third series was likewise commenced at the end of six months. In table No. XIV, the mortars of the first series, made of lime slaked to powder, were formed at appropriate periods, which were not extended beyond seven months. The resistances obtained in table No. XIV are not the same as those obtained in Table No. XV, because the proportions were different: but if we compare the experiments of the first series of table No. XIV with those of the first and third series of table No. XV, we shall see that instead of making hydraulic mortar of fat lime, sand and trass, immediately after slaking the lime, it will be more advantageous, after slaking the lime, with a little water, as it comes from the kiln, to leave it exposed to the air for some time. We see, here again, that the hardening was more slow in winter than in summer. If we compare the results obtained in the preceding tables, we shall see that there is a great difference in the effects of mortars made of natural or artificial hydraulic lime and sand, and of those made of fat lime, sand and trass. When the first are made of lime which has for some time slaked to powder, or which has been air-slaked, they generally lose much of their force. There is not the same disadvantage with fat lime: whether the mortars are made as soon as the lime comes from the kiln, or after it has been slaked with a little water and left exposed for some time to the air, or after the lime has been air-slaked, good results are always obtained: but we have seen that the best are got by slaking the lime with a little water as soon as it is burned, and leaving it exposed for some time to the air in a covered place. Experiments, to be given by and by, will show that I obtained good results, also, by making hydraulic mortars of sand, trass, and fat lime which had been lying wet in basins for four or five years.

We have also seen by comparison of the preceding table, that mortars made of hydraulic lime, natural or artificial, without trass or puzzalona, did not harden, with sand, until from eight to fifteen days, although giving good resistances; while those made of fat lime, sand and trass, hardened at the same season of the year, in the space of from four to six days, and, on the average, gave much greater resistances.

These results induced me to make researches in order to the fabrication of factitious trass or puzzalona. In the following article I shall give the results I obtained—commencing with several essays that have previously been made by others, with this view.

[TO BE CONTINUED.]

Mechanics' Register.

LIST OF AMERICAN PATENTS WHICH ISSUED IN MARCH, 1837.

With remarks and Exemplifications by the Editor.

25. For an improvement in the *Platform Balance;* Benjamin Ball, city of New York, March 3.

This balance in its general arrangement, is like many others of the platform kind; the particular points claimed would require the drawing for their illustration; they do not appear to be of very great importance, but present enough of novelty upon which to claim a patent.

26. For an improvement in the *Blow-pipe for Blast Furnaces* of all descriptions; John Barker, city of Baltimore. Patent issued February 12th, 1836; re-issued on an amended specification, March 3.

The specification of this patent need not now be given, as the invention is the same with that described at page 281, as patented by John J. Hawkins, of London, Mr. Hawkins having been the agent of Mr. Barker in this business; and in England patents are always granted to some person within the realm, and issued, therefore, in the name of the agent if the inventor is not present.

27. For an improvement in the *Machine for Sawing Staves;* Jesse J. Smith, Brutus, Cayuga county, New York, March 3.

In this machine the staves are to be sawed by means of a barrel saw, attached to a vertical shaft, the teeth of the saw being downwards. Within it there is a stationary block of wood which nearly fills the interior space, but yet allowing the saw to run clear. Four spaces are cut from the bottom to the top of this block, to receive the staves to be sawed, the width and depth of these spaces corresponding therewith. The pieces of plank from which the staves are to be sawed, after being cut to the proper length, are fixed upon four carriages which are so constructed as to feed it upwards to the saw, in the proper curved direction; the staves, as they are cut, passing into the spaces prepared for them in the interior block, which, when they are separated from the log, prevent their being carried round by the saw. There are contrivances for the self-setting of the stuff on the carriages, and the apparatus appears to be generally well arranged.

The claims are to "the placing within such a saw a permanent block of wood, grooved or channelled to receive the staves to be cut, in the manner described. The position and mode of running the saw, in combination with the stationary bulged block within it, separately and distinctly from the carriages and their appendages, as constructed."

28. For an improvement in the *Mode of Moulding Candles;* John Moore and Samuel P. Bawer, Strasburg, Lancaster county, Pennsylvania, March 3.

This patent appears to be taken for an improvement upon the mode of moulding candles, invented by a Mr. Yard. There may be thirty-two moulds, or any other desired number fixed in a frame, and the candles are to be drawn from the lower ends of the moulds; the improvement consists, mainly, in four strips of wood which are to confine the wicks, form the trough for pouring the tallow, and co-operate with a stationary sliding frame in drawing the candles. The apparatus is not well described, but possibly a reference to Mr. Yard's patent might render all clear. The claims are to the manner of arranging the strips above spoken of.

29. For a *Register and air box for admitting the air to Fire Places;* Allan Pollock, Boston, Massachusetts, March 3.

This is a contrivance intended to regulate the admission of air from without the room, to feed the fire contained in a grate. The box described may be made of cast iron, and it is to be built into the chimney in setting the grate, in such a way that the bottom of the box shall rest upon the hearth, its front edge being even with the back of the grate, and extending up from the hearth to the grate, thus occupying the ordinary position of the chimney back under the grate; this front side of the box is entirely open. The depth of it from front to back may be about six or eight inches, the back plate is to be perforated, near its lower edge, with four holes, more or less, say of two inches in diameter, which holes may be simultaneously opened or closed by a sliding register, allowing air from without the house, or from a cellar, entry, &c., to pass through the openings in regulated quantities. A lever attached to the register extends to the front of the fire place, serving to move it at pleasure. To prevent ashes, &c., from coming into contact with the register, or from being blown into the room by the entering draught, a partition extends from the bottom along the whole length of the box, about midway of its depth, and rises to within two inches of its top.

The claim is to "the register with two or more spaces, and apertures, being so placed that by moving the register the corresponding apertures in the back of the box may be opened or shut simultaneously; also the air box with the middle partition intended to change the direction of the current of air so as to prevent the dust and ashes from being blown into the room."

30. For a *Machine for Breaking Ice,* to keep channels open during the winter; Barnabus S. Gillispie, city of New York, March 8.

A shaft is to extend across the bow of the boat. and is to be made to revolve by means of a steam engine. Upon each end of this shaft there is to be a circular saw to cut the ice, and along it there are to be jointed hammers, which by the revolution of the shaft are to break the ice so cut. The claim is to "the application of jointed hammers, or beaters, attached to the revolving shaft, in combination with the circular saws."

We doubt the efficiency of the above proposed means. The velocity with which the shaft is made to revolve cannot be such as to give much power to the hammers from their centrifugal force; their revolution with the shaft, amongst the ice must, we apprehend, be a source of great resistance and expend a large portion of the power of the engine.

31. For a *Spring Seat Saddle*; Robert Wilson, Milton, Northumberland county, Pennsylvania, March 11.

The claim is "the application to a cross straining, applied to the common riding saddle, springs which will give the cross straining the desired motion, and allow the webbing which passes under it, to rise and fall in such a manner as is required to make a pleasant seat to ride upon, using for that purpose any kind of materials which will produce the intended effect."

The springs, it is said, are so made and fixed that in case of breaking, or other injury, they can be easily replaced by others, the padding only being removed for that purpose.

32. For a *Machine for mortising Posts, and Sharpening rails for Fences;* Wandle Mace, city of New York, March 11.

This machine bears a resemblance in several particulars to some others which have been constructed for a like purpose; its main merit consisting in the manner in which the parts are combined together so as to effect the mortising and sharpening in one machine. The post to be mortised is placed upon a carriage, and the boring is effected by, two, three, or more augers, operating simultaneously; when, by shifting the carriage, the bored part is brought under chisels in a frame vibrating vertically, which chisels remove the superfluous stuff. By means of curved chisels, or cutters, placed in the same frame the rails are to be pointed so as to fit the mortises. The claim is confined to the particular mode of construction, as set forth in the specification.

33. For improvements in *Pumps* for raising water; Samuel Adams, Townsend, Middlesex county, Massachusetts, March 11.

Improvements, so called, in pumps, we have frequently had occasion to remark, if real, are usually very small, and to this, the case before us forms no exception, whether novelty or utility be considered. The things described are, first, the mode of attaching a leaden tube to the lower end of a wooden pump, to lead down into the water; we are apprehensive that, taking all things into account, wood will be most generally preferred to lead for this purpose. Secondly, a mode of opening the lower valve, so as to let out the water from below it, to prevent its freezing. This is to be effected by raising the pump handle to the greatest possible height, which depresses the piston, and brings it into contact with a projecting tail on the lower valve, which it tilts, and opens. Our mechanical philosophy will not enable us to perceive how this is to effect the discharge of the water, as there is not any contrivance for simultaneously opening the upper valve, a thing which we should deem necessary to the discharge of the water below, as well as above it. There has been a patent obtained for a pump in which both valves were to be opened, in a manner very similar to, though not identical with, the foregoing. This plan cannot be carried into operation in the way proposed without placing the upper and lower boxes close together, an arrangement not always to be preferred.

The claim made is to "the connecting of lead and other pipes with wooden pump barrels so as to form an air-tight junction, by means of locating the lower box in a socket connected with a pipe as aforesaid within the pump barrel, and other means as aforesaid; and the letting off the water to prevent freezing, by means of the lever valve in the lower box, opened by the projection in the bottom of the upper box, all as aforesaid."

34. For improvements in *Cooking apparatus;* William W. Parrott, Boston, Massachusetts, March 11.

This apparatus is, mainly, an open grate, or range, with ovens in the jambs, and a platform behind and above the level of the fire, upon which cooking utensils may be placed. The claim is to "the oven constructed, placed, and heated, as above described, with such a grate as above described, with such a platform at the top of the grate, and ovens as above described." In every thing which is progressive, and which in its progress slowly changes its character, it is always difficult, and sometimes impossible, to mark the precise point of change. Such, manifestly, must be the case with many of the inventions brought before the Patent Office; a strong doubt frequently exists whether a patent ought to be granted, and not unfrequently this doubt is all that turns the scale in favour of the applicant; the foregoing was a case in point, and the claim, therefore, was limited to the special mode of construction, *as described.*

35. For an improvement in the *Truss for the cure of Hernia*; Elijah Jaquith, Brattleboro', Windham county, Vermont, March 11.

The improvement claimed consists merely in the arrangements made for causing the pad to bear against the ruptured part by the action of a helical spring. The mode proposed is equally good with many others which have preceded it, and the arrangement of the parts is believed to be new.

36. For improvements in the *Scythe Sneath;* Dexter Pierce, Montague, Franklin county, Massachusetts, March 11.

The claim is to "the constructing the nib or thole irons and woods so as by the extension of the iron beyond the wood, with a screw and nut to regulate and fasten the nibs, or tholes to the sneath, in any situation desirable, and the method of bushing and staying the spotting, as set forth."

The improvements may be very useful, but they are not of a kind to justify an extended description in our pages.

37. For an improvement in the mode of *Dressing the stones of portable and other mills;* Nathan Jacobs, Newark, Licking county, Ohio, March 11.

The claim made will sufficiently set forth the nature and extent of the improvements; it is to the "dressing a large portion of the face, towards the periphery, perfectly flat, while the other part, interior to this is slightly concave and furrowed in the manner set forth."

38. For a *Rail-Road Stop;* Thomas J. West, Whitehall, Caroline county, Virginia, March 11.

What is called a rail-road stop, is a pit, or excavation constituting an open space between and under the track rails, on each side of the crossing of a lateral road. These pits are to be at least three feet wide, which width, it is supposed, will be sufficient to prevent cattle from jumping across them. The depth is to be at least two feet, but in situations where the road is elevated above the surrounding ground, the depth need not be limited. Suitable supports are to be placed underneath to sustain the track, and to give permanence to the structure. Lattice work, it is stated, may be placed above the pit; the openings in which must necessarily be such as will prevent horses, or other animals from walking over them. The fence of the lateral road must, of course, reach to the pit, or excavation under the rails.

"The form and shape of said open bridges, or road stops, may be made in any mode which the convenience or fancy of the constructor may suggest; the petitioner claiming the principle herein set forth, and its adaptation to the invention, as the ground of his prayer for a patent."

39. For improvements in the mode of *Constructing and loading fire arms;* Thomas McCarty, Elmira, Chemung county, New York, March 11.

The main feature of this invention is the employment of a metallic tube to contain the load, which tube is to be slipped in at the breech of the gun, such tube having a lateral tube, or arm, at its back end which, when in place, projects beyond the barrel so as to receive a percussion cap. When the gun is to be loaded, a stirrup, which swivels upon pins on the side of the barrel is raised, which admits of the removal of the discharged tube, and the substitution of a new one.

The claim is to "the manner of loading the gun by the use of a tube containing the whole charge, with the arm reaching out so as to be fired by an outside lock, or otherwise; which tube remains until the load is discharged, then to be replaced by another similarly loaded; and also the manner of securing the tubes by means of the stirrup. This invention is equally applicable to the rifle, the musket, the shotgun, pistols, or cannon."

It was known in the office, when this patent was granted that tubes for containing the load, and which might be carried as catridges, had been used in guns of a particular construction; it is now believed, however, that they had been employed in various ways, which were not then known, The patentee, it will be seen, is limited to his own mode of application, and this possibly may be new. Whether the plan will wear well, however favourably it may be thought of at first, may be fairly doubted; it will require no little care to prevent corrosion and foulness, to an extent which will interfere seriously with the use of the tube in such a gun.

40. For a machine for *Making and turning the heads of Wood Screws;* Thomas W. Harvey, Poughkeepsie, Dutchess county, New York, March 25.

It would require very complex engravings to make known the construction of this and the next following machine; the claims made refer to points of construction not to be understood from verbal descriptions, and it would be useless, therefore, to insert them. The main object of the present machine, is to carry on the nicking and turning simultaneously, which operations have heretofore been performed separately, and in distinct machines.

41. For improvements in a machine for *Cutting and Heading wire,* for the manufacture of wood screws, Thomas W. Harvey, Poughkeepsie, Dutchess county, New York, March 25.

42. For improvements in the *Machine for Sawing Shingles, Clapboards, Laths, and various other species of lumber*; Apollos Wilbur, Burrilville, Providence county, Rhode Island, March 25.

In this saw mill there are various contrivances for sawing stuff by single saws, and by gangs of saws, as may be required. For sawing shingles and clapboards, there is a double carriage to sustain the stuff, the upper carriage rocking upon the lower for sawing feather-edged stuff; and also working upon a pin so as to allow the upper carriage to be placed obliquely to the lower, longitudinally, to give the requisite slope to shingles, &c. There is also a saw for cross-cutting, which when not in use, stands in a vertical position, but can be brought down at any time, to cross-cut the stuff, into the required length. The claims made are, generally, to the particular modes adopted of effecting the proposed ends. But besides these, there is a claim to "the particular manner of forming head and tail blocks in three or more thicknesses," in which the stuff is to be cut from plank, the grain crossed and the whole bolted or pinned together; there is likewise a claim to dogs for holding the stuff, consisting of tubular bolts, the holding ends of which are formed into teeth, to be driven into the log, and which, it is believed, will hold firmly without tending to split it; there are some minor claims, which we pass over.

43. For an improvement in *Spring Saddles*; John G. Manlove, Bainbridge, Ross county, Ohio, March 25.

A bow formed spring is attached to the head of the saddletree, extending forward to the distance of six inches or more. The straining girth is attached in front to a roller affixed to this spring, and extending back in the usual way, the claim is to the combination of the spring with a horn, or upright which covers and in part sustains it, with the roller in the way described; which way is not very clear.

44. For an improvement in the process of *Burning Lime*; Samuel Garber, and H. Swartzengrover, Norristown, Montgomery county, Pennsylvania, March 25. (See specification.)

45. For improvements in *Stove Pipes and Drums*; Merrit Bradford, Saugerties, Ulster county, New York, March 25.

This invention is intended to increase the heat given out by stove pipes and drums, by arresting it in its passage to the chimney. The method adopted is the insertion of a number of tubes in the pipe, crossing it from side to side, and open at both ends, so that the air of the room may pass freely through them, and become heated. The tubes may cross the axis of the pipe, or there may be two or more in the same plane of its cross section, "by which means" says the patentee, "currents of air will be established; the surrounding atmosphere, by the known laws of nature, will enter the lower extremity of the tube, become heated in its passage through the tube, and pass off at the upper extremity." The claim is to "the insertion of said tubes into stove pipes, and drums, perpendicularly, horizontally, or at any angle which may be desired."

We are very apprehensive that the known laws of nature will do but little in causing currents of air to pass through these cross tubes; we judge not only from some acquaintance with the particular law referred to, but from our practical knowledge of the little effect produced by some analogous contrivances.

46. For improvements in the *Machine for Cutting the threads of*

Wood Screws; Jacob Sloat and Thomas Sprengsteen, Ramapo, Poughkeepsie, Dutchess county, New York, March 30.

This machine is in the same predicament with those referred to in No. 40 and 41. That is to say, any attempt at verbal description, would lead us far beyond the mark at which we aim, and would be read by few, and by fewer comprehended without the drawings accompanying it.

47. For an improved *Machine for Cutting Brads and Nails;* O. P. Randand Joseph Berry, Newmarket, Rockingham county, New Hampshire, March 30.

This machine is to cut two brads by one revolution of the crank shaft which works the cutters, two of which are placed near together in the same slide. The plate to be cut is fed up by two rollers which receive their intermitting motion through the intermedium of a rag wheel, and feed arm, moved by a crank shaft. The two cutters are so placed as to cut the brads alternately to the proper slope, in reversed directions, and out of each other. The claim is to the particular arrangement described.

48. For improvements in the machinery for *Packing Flour in Barrels*; Oliver Jewell, Rochester, Monroe county, New York, March 30.

A rod, working vertically, and having a packing piston on its lower end, is common to this and many other packing machines; the proposed improvement, consisting principally in the manner in which the piston and its rod are worked up and down, which is effected by an eccentric groove in the face of a wheel, which wheel revolves on a strong shaft; the piston rod having a slot, to allow the shaft to pass through it, whilst the rod may still work freely up and down. The wheel which is to work the piston may be two feet in diameter, and the groove in its face, a circle of sixteen inches so cut as to receive a friction roller which is firmly affixed to the rod, just above the slot. The circular groove is to coincide at one point, with the periphery of the wheel, and when the latter revolves, the piston will rise and fall to the distance of eight inches. Where the two peripheries coincide, there is an iron door, shutter, or slide, which may be opened to let the piston roller pass out of the groove, the wheel may then continue to revolve without acting upon the piston. This wheel may be placed in an apartment above the packing room.

Claim. "I do not make any claim to the several individual parts which I have described, as they constitute parts of other machines, for the same or other purposes; all that I claim as new, and wish to secure by letters patent, is the application of an eccentric groove, upon the face of a revolving wheel, to work the rod and piston of a machine for packing flour; the whole constructed, combined, and operating, substantially in the manner herein set forth."

We have heard a very good character of the operation of this machine, from those who have seen it in actual use; a much higher, indeed, than we had anticipated.

49. For a improvement in the spring for *Spring Seat Saddles;* Henry Sheetz, Staunton, Virginia, March 30.

If the rider becomes galled in his seat, it will not be because there are not spring saddles in sufficient variety, which we are assured are patent preventatives. Some of them, it is true, bear so near a resemblance to each other, that a choice between them may be a matter of some difficulty, and one in which we can render but little aid from the very circumstance of their near resemblance. The spring in the saddle of Mr. Sheetz, is to be of the zigzag kind, attached to a frame, of a peculiar construction, and acting by means of the straining girth, in a way sufficiently different from others, to establish a claim to novelty. He tells us that, "the advantages which this spring has over all others in use, is the great strength and permanency, combined with elasticity, of the zigzag spring, secured to it by machinery, regulating the action of the spring, occupying but little space, and being easy of construction and durable, I do not claim the zigzag spring as my invention; but what I do claim, is the improvement produced by the invention and application in the mode above specified, of the double sliding frame, regulating the action of the spring."

50. For a *Straw Cutter;* Henry Silliman, Perry Village, Gennessee county, New York. March 30.

This straw cutter is peculiar in its construction, presenting in this respect more novelty than is usual in machines for this purpose. The part in which the straw is to be placed, consists of a hollow cylinder standing horizontally upon a suitable frame. The interior of this cylinder is to be divided into several compartments, say four, by partitions running the whole length of it. The cross section of each compartment, would in this case, be a quadrant; but the inner angle of this quadrant is cut off in consequence of the passing of a screw, through a tube or case occupying the axis of the cylinder. The cutting knives are fixed upon the face of a wheel at one end of the cylinder, which is open, and as this wheel revolves, the screw with which it is connected, also revolves, and carries a nut which moves back and forth within the centre tube. From this nut, rods project out at the back end of the cylinder, which rods are received and attached to pistons, or followers, which fill the compartments, and serve to force the straw forward. Each compartment is furnished with a door, which is opened to supply the straw when requisite. The cylinder may be turned round in its frame, to bring the doors successively to the top.

The claim is to the manner of acting upon the pistons, by which the straw is fed to the cutting knife, or knives, by the intervention of a revolving screw, carrying a nut backward and forward, and with it the rods and pistons, substantially in the manner and for the purpose set forth.

51. For improvements in the *Metallic Pistons for Steam Engines, Pumps, &c.* John Swainson, City of New York, March 30.

The main object of this improvement is to allow the steam, or other fluid, to pass into the piston, between the upper and lower plates, so that it may act upon the interior of the packing with a force equal to that of the pressure on the outside. The application of this principle is not new, having been made the subject of a patent by Mr. Hall of Bassford, in England; it is therefore, to his particular mode of effecting this, that the patentee of this improvement is confined. The claim is to "the mode of placing a middle plate in the piston head, as described, and the combination therewith of the two outer plates, which contain openings through which the steam may pass in behind the whole inner surface of the packing, and press it out against the cylinder, with a force equal to the pressure of the steam or other fluid."

The piston is described as consisting of three plates, a middle one which is solid, and always retains its place, and a lower and upper plate, adjustable by screws, each perforated to admit steam between it and the middle plate. The packing is to consist of metallic rings, breaking joints, and from the mode of fixture the packing of each segment, is pressed by the steam alternately at the return of the stroke.

SPECIFICATIONS OF AMERICAN PATENTS.

Specification of a Patent for an improvement in the process of burning lime; granted to SAMUEL GARBER AND HENRY SWARTZENGROVER, *Norristown, Pennsylvania, March 25th,* 1837.

To all whom it may concern, be it known, that we Samuel Garber and Henry Swartzengrover, of Norristown, in the State of Pennsylvania, have invented an improvement in the process of burning lime; and that the following is a full and exact description thereof.

We take a kiln of either of the ordinary constructions for the burning of lime, and we adapt thereto any of the various kinds of blowing apparatus used for forges, or for furnaces, and after having charged our kiln with limestone and fuel in the usual way, and ignited the fuel, we introduce a blast of air into the same, giving to the blast such a degree of force as may be found requisite. The blowing apparatus which we prefer, is the ordinary fan-wheel, as it affords, at once, a more diffused blast than that from bellows, or from blowing cylinders: but by the adoption of proper means, well known to machinists, as by blowing into a regulator, and by introducing the blast through several openings as large as may be found convenient, the blast may be sufficiently diffused from any blowing apparatus.

It is well known to those persons employed in the business of burning lime, that the operation of a kiln is much influenced by the state of the weather, the fuel sometimes burning too feebly to produce a sufficiently high temperature, but by means of the artificial blast, the process is made to go on independently of the atmospheric changes, and the proper temperature is rapidly attained. By this procedure, the saving of fuel is large in amount, as the proper degree of heat can not only be produced but also maintained, until the calcination is completed; and furthermore, we are enabled to burn such fuel as would either be thrown away, or would be rejected as unsuited to the burning of lime in kilns without the blast.

We do not claim as our invention any particular kind of blowing apparatus, nor any particular manner of constructing a lime kiln, but employ any of those now in use, which may be adapted to our purpose; all that we claim being the introduction of wind into a lime kiln, by a blowing apparatus, for regulating the combustion of the fuel and, in consequence, the calcination of the lime.

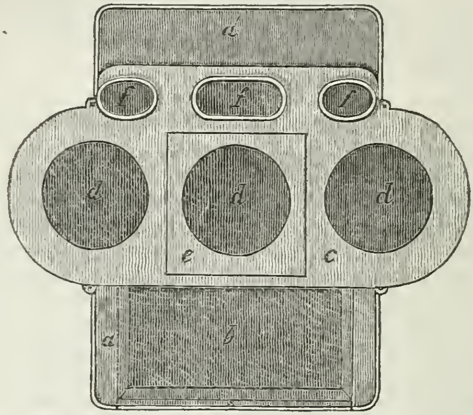
SAMUEL GARBER,
HENRY SWARTZENGROVER.

Specification of a Patent for an improved Cooking Stove; granted to JAMES N. OLNEY, *City of New York, November 20th,* 1837.

To all whom it may concern, be it known, that I, James N. Olney, of the City of New York, in the State of New York, have invented certain improvements in stoves used for cooking, which though intended principally for anthracite, may be used with other coal, or with wood; and improvements also in the manner of constructing and combining an elevated oven therewith. And I do hereby declare that the following is a full and exact description thereof.

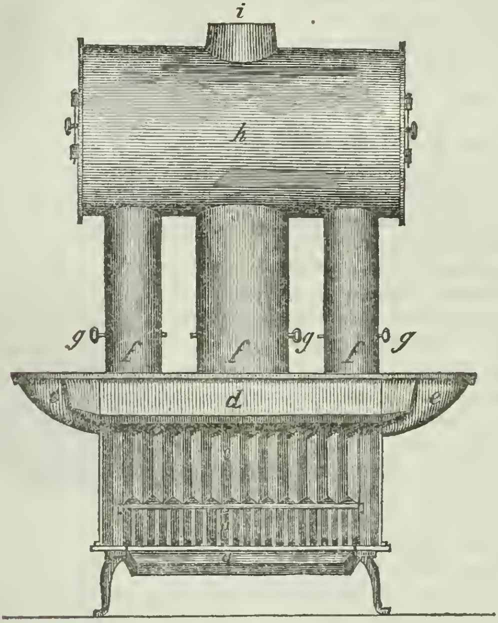
Fig. 1 in the accompanying drawing is a plan, or top view of my stove, the elevated oven being removed. Fig. 2 is a front elevation of the stove, with the elevated oven. Fig. 3, an end view of the same, and Fig. 4, a vertical section thereof through the middle, from front to back.

Fig. 1.



The bottom plate of this stove extends out in front so as to form a hearth, *a*, fig. 1, with a sunken ash pit *b*, as in many other stoves; it has also a hearth *á*, projecting out at the back, serving to sustain a tin roaster, or baker, which may be placed in the recess formed by the projecting of the end plates, and of the top of the stove,—*c*, is the top plate, having boiler holes *d*, *d*, *d*; the centre one of which is made in a square plate *e*, which may be removed for the purpose of placing a gridiron over the fire, which gridiron may have a tin cover, with a pipe to conduct the fumes into one of the openings or flues of the stove. The openings *f*, *f*, *f*, are to receive draught pipes, which communicate with the elevated oven, shown at *h*, fig. 2 and 3. In this latter figure *a*, is the ash pit, and *b*, the front bars of the grate, forming the lower part of the chamber, which is to contain the fuel; these grate bars have an elevation of about four inches, extending up to the corrugated, or zig-zag plate *c*, which forms the front of the fire chamber, its lower edge being five inches above the hearth. One of the grate bars is seen at *b*, in the section, fig 4. The end plate *b*, fig. 3, is formed with a recess, to receive a lining of fire bricks, and the back also is lined as shown at *e*, fig. 4, and the back plate being recessed for that purpose. In this last figure, *a*, is the fire chamber, which with the back lining *b*, and the zig-zag, corrugated plate, seen in profile at *c*, forms the whole depth of the body of the stove, which it will be seen is much less than the width of the end plate *b*, fig. 3. This extra width is occupied by a front and a back recess, formed by the hearths, the projections of the end plates, and of the top of the stove, thus contracting the fire chamber within proper limits. The vertical edges of the end plates are shown by the lines *k*, *k*; and the manner in which the top projects in front and back is shown at *d* and *g*, which form spaces above the fire chamber, the front projection being shown also at *d*, fig. 2. This space is again enlarged at each end, by the swells *c*, *c*, on each of the end plates (figs. 2 and 3.)

Fig. 2.



The elevated oven *h*, fig 2, I make cylindrical, forming it of two concentric plates *h*, *h*, fig. 4, into the space between which the pipes *f*, *f*, *f*, (figs. 2, 3, and 4) lead. These pipes are furnished with dampers *g*, *g*, to regulate the heat. In stoves of a small size, two outer pipes *f*, *f*, will suffice, but in the larger kind it will be found best to have three, as giving a more perfect command of the heat. These pipes should elevate the oven above the ordinary height of cooking utensils, which will in general be effected by a length of about fourteen inches. The collar *i*, on the top of the oven, should be placed in the centre, as shown in the drawing, as the heat may then be best regulated.

The ends of the oven I make of cast-iron, the cylinders being of sheet-iron. A cast-iron rim, or ring, is furnished with flanches which receive the cylinders, rods passing through the spaces between the two cylinders, to confine the rims in their places. At each end, occupying the spaces within the rims, I place a cast-iron door, provision being made for suitable hinges and latches; a shelf, or plate, of sheet-iron, *j*, fig. 4, which should be perforated with holes, stands an inch or two below the centre of the oven; this can be removed when desired.

The sliding plate which covers the ash pit, or sink, in the front hearth, is so constructed that a flap attached to it may be made to cover the grate bars in front, so as to convert the fire chamber into a close stove, and this is combined also with a sliding shutter, or damper, causing this apparatus to operate as a blower, and to regulate the draught with the utmost nicety. This arrangement is shown in figs. 6 and 7.

Fig. 5 is a top view of the sliding plate, and *a*, *a*, *a*, are openings through it, towards its back edge: instead of several openings, as represented, there may be one only, of any required length. A sliding damper, furnished with a rod, which may extend forward under the sliding plate, serves to regulate this damper, so as to open or close the aperture, or apertures, to any extent. To the back edge *d*, of this sliding plate there is hinged a flap, which, when down, covers the openings *a*, *a*, and their damper, and is of such width that when up it shall reach to the corrugated plate, completely enclosing the grate bars. This flap is shown at *b* in the edge view, fig. 7; when down it falls into the recess *c*, so as to be flush with the general surface of the sliding plate. Fig. 5 is a section of what I have called the zig-zag, or corrugated plate.

What I claim as my invention in the within described stove and elevated oven, is the constructing of the stove with the combined front and back recesses, formed by the two hearths of the bottom plate; the projection in front and back of the two end plates, and the widening of the fire chamber, immediately under the top plate, as herein fully set forth. I claim also the arrangement herein described, by which the grate bars in front may be

Fig. 3.

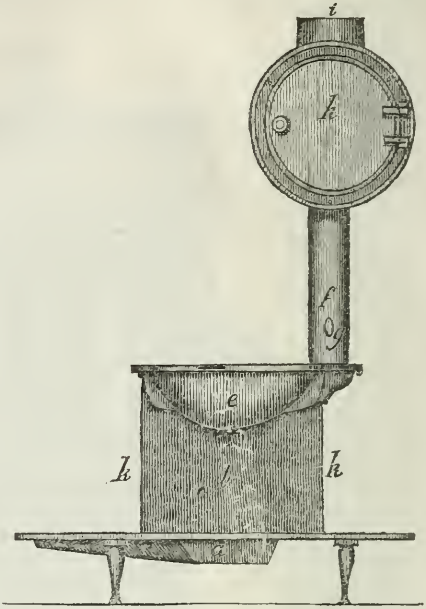
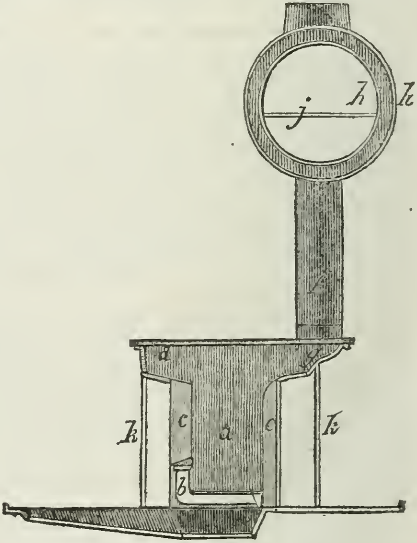


Fig. 4.



enclosed by means of a plate, or flap, hinged on the back edge of the sliding cover of the ash pit, and the combination therewith of a sliding damper, covering openings in said sliding cover, substantially in the manner and for the purposes set forth. I do not claim the forming and placing of an elevated oven above a stove, with pipes or flues leading therefrom to the space surrounding the oven, this having been before done; but I do claim the combining of a stove such as is herein described, with a cylindrical oven, having doors at each end, the whole connected and arranged as above shown, such construction and arrangement being as I believe, essentially new. James N. Olney.

Fig. 5.

Fig. 6.

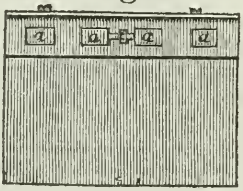


Fig. 7.

Franklin Institute.

Quarterly Meeting.

The Fifty-fifth Quarterly Meeting of the Institute was held at their Hall on Thursday, October 19, 1837.

Mr. John Gilder was called to the chair, and

Mr. John S. Warner appointed Recording Secretary, pro. tem.

The minutes of the last meeting were read and approved.

Donations of Books were received from Messrs. Carey & Hart; the Philadelphia Library Co.; Edward C. Biddle; B. H. Hollingshead; Prof. A. D. Bache, of Philadelphia; Western Academy of Natural Sciences, St. Louis, Missouri; Thadeus B. Wakeman, Esq., of city of New York; James E. Mitchell, Esq., of Harrisburgh, Penn.

Isaiah Lukens, Esq. of Philadelphia, presented specimens of Iron Ore.

Messrs. E. & T. Fairbanks of St. Johnsbury-plains, Vermont, presented a scale for the use of Druggists, Grocers, &c., graduated to half ounces.

The Actuary laid on the table the periodicals received in exchange for the Journal of the Institute during the past quarter.

The quarterly Report of the Board of Managers was read and accepted; and on motion, referred for publication.

The Treasurer presented his Quarterly Report of the Finances of the Institute which was read and approved.

(Extract from the minutes.)

John Gilder, *Chairman.*

John S. Warner, *Rec. Sec. pro. tem.*

The fifty-fifth quarterly report of the Board of Managers.

In presenting the fifty-fifth quarterly report, the Managers have the gratification to state that, although the period embraced by it has not been marked by the commencement of any new undertakings on the part of the Institute, the several sub-divisions of scientific and practical subjects placed in charge of the Committees of the Board, have received all the encouragement that the means of the society would allow.

The interesting investigations of the causes of explosions in steam boilers, have been concluded, and the general government and the community, put in possession of the valuable results of these experiments and ample directions and cautions, tending to prevent the frequency of such lamentable accidents.

The final report of the committee, on the value of water as a moving power may be expected at an early meeting of the Institute, and the practical mechanic will have no cause to regret the delay, which has attended its completion, when he reflects on the vastness of the subject under examination. and the more than imperfect theories, heretofore, at his command, in the published treatises relating to it. The experiments made under the authority of the Institute, were of the most perfectly practical character, and while no attempt will be made to sustain a preconceived theory in relation to the best mode of applying water as a moving power, the applications of a sound theory, deduced from actual practical results, will be put at the command of those interested in the enquiry.

The committee on Science and the Arts, have been sedulously prosecuting the inquiries and examinations requested of the Institute, by inventors and others whose pursuits are benefitted by such investigations, and the Board refer to their report herewith presented, for a detailed exhibit of their transactions.

The committee on the New Hall, have not yet been able to obtain a sufficient amount on loan to warrant the removal of so much of the Masonic Hall, as may be necessary to commence the construction of a new edifice for the accommodation of the Institute; but although disappointed in their reasonable expectations, they are not yet so much dismayed with the prospects before them, as to recommend any thing but renewed and cordial exertions, to accomplish an object of such vital interest to the Institution. Up to the present time, at a comparatively small sacrifice of the active means of the society, all the pressing demands connected with the purchase have been met, and the Board, on the recommendation of the committee, have directed subscriptions to be received for the Stock of the loan, heretofore authorized, in such sums as will be found entirely convenient to every member of the Institute. The Board invite the attention of the members to the plan, as at once combining the advantages of a safe and productive Saving Fund, and enlarging and sustaining an institution, devoted to the cause of sound science, and the promotion of the mechanic arts.

The department of instruction, has received that attention which its important bearing on the stability of the Institute demands. Arrangements are in progress, for the commencement of the regular courses of Lectures on Chemistry, Natural Philosophy and Mechanics, and on the applications of science to various arts and trades.

To these the Board at present have the prospect of adding a series of volunteer lectures, on such collateral branches ot science as are of general interest, and they hope that the exertions of the committee, will be met by a corresponding attention in the members.

The course on Chemistry, will be conducted by Professor Mitchell, whose valuable aid in that department we have now enjoyed for several years, and at a sacrifice to his personal interest, which can only be compensated by the satisfaction we know he feels in sharing with his fellow citizens his vast and profound attainments in the beautiful science to which he is devoted.

By the resignation of Professor Johnson, the Institute has lost a zealous teacher of Physical Science, whose appointment to an important station in the Exploring Expedition, now being fitted out by the general government, will afford an ample field for the exertion of his industry and research, which we have so long enjoyed in our connection with him as an associate and Professor.

The department of Natural Philosophy and Mechanics, has been confided to Mr. John C. Cresson, a gentleman already favourably known to many of the members, and from whose devotion to these practical branches of education the Board anticipate a success honorable to himself and advantageous to the Institution. Mr. James C. Booth, whose valuable services we have enjoyed for the past two years, will continue to conduct the lectures on the applications of Science to the Arts; the department is one which the Board is most anxious should command the attention of the members, as it brings down to the actual walks of life, those almost unseen connections, which profound science maintains with manufacturing and domestic industry, and the extensive cultivation of a knowledge of this connection must lead to great improvements in what is now the mere routine of the factory and workshop.

The Drawing Schools of the Institute, were opened on the 17th inst., the department of Mechanical and Miscellaneous Drawing, under the care of Mr. William Mason, and that of Architecture and Practical Carpentry, in charge of Mr. John McClure. Both these gentlemen are teachers whose efficiency in their profession have been fully tested by the experience of the Board, and they have no hesitation in recommending them to all who desire instruction in the branches they teach. The accommodations for these schools, have been much enlarged and they are possessed of every convenience for the reception of a numerous class.

In addition to the usual augmentations of the Library, by purchases and exchanges, about one hundred volumes have been received from our esteemed member, Professor Bache, President of the Girard College, now in Europe. These books have been purchased out of a fund, placed in in the hands of Dr. Bache by the subscription of the members, and having been selected by a mind distinguished for its discrimination and devoted to practical science, are great acquisitions to our collection. Some additions have been made to the Cabinets of Models and Minerals; they are gradually obtaining their proper hold on the attention of the public, which leads us to hope that at no very distant day, they will fully correspond with the success of the Institute in its other departments.

The Treasurer's report of the receipts and expenditures of the Institution are herewith presented, for the information of the members.

During the quarter forty-one new members have been elected, and two resignations received. Of the new members thirty-one were associated with the Institute, by the the incorporation of the Mechanics and Tradesman's Exchange; the union of the two societies, reported at the last quarterly meeting as about to take place, having been since accomplished, and a large and valuable accession of strength secured to the Institute, by connecting with it some of our most respectable tradesmen and mechanics, and giving an additional warrant for the perpetuity and success of our association.

JOHN STRUTHERS, Chairman.

WILLIAM HAMILTON, Actuary,

Third Monthly Conversation Meeting.

The Third Monthly Conversation Meeting of the season, was held at the Hall of the Institute, on Thursday evening, November 23rd, 1837.

The meeting was numerously attended by members and invited strangers, who availed themselves of the opportunity for social intercourse and the informal discussion of scientific and mechanical topics.

There was a very respectable display of articles of American manufacture and several newly invented machines and implements.

From Messrs. B. Slater & Co. were sent a variety of weaver's shuttles and temples, some of which are of improved construction. Also an improved satinet shuttle, broad cloth and power loom shuttles from the same gentlemen, and a governor for water wheels of compact form.

Messrs. W. H. Carr & Co. exhibited some neat shutter fasteners, made by M. M. Israel & Co. of Westfield, Con., and samples of pump augers, very beautifully finished, from the well known manufactory of N. P. Ames, of Cabotville, Springfield, Mass.

Messrs. Curtis & Hand produced samples of hollow and common screw augers of various sizes, and some highly reputable specimens of table cutlery manufactured by Messrs. J. Russell, of Greenfield, Mass.

Mr. A. C. Jones laid before the meeting a drawing of the boiler of the steam ferry boat which exploded a few weeks since at Camden, N. J., together with a part of the boiler, affording remarkable evidence of the destructive effects of explosive steam.

Mr. J. J. Hitchcock exhibited a working model of Green's patent straw cutter.

Messrs. Gleason & Griffith presented a parlour stove of neat appearance.

Mr. Jos. Harrison, Jr., presented a grate bar which he has recently devised for locomotive engines consuming anthracite. The bar is formed with a longitudinal groove for the reception of a shield of fire clay.

A machine for reeling and spinning silk at one operation was exhibited by the inventor, Mr. Adam Brooke, of Scituate, Mass. This machine has been submitted to the Committee on Science and the Arts, from whom a report may shortly be expected.

Mr. Joseph Saxton put in operation a very compact and active electro-magnetic machine of his own invention and construction.

Mr. Samuel Hufty exhibited a very pretty philosophical toy, misnamed a kaleidophone, which displays several beautiful figures formed by the vibration of wires of different forms.

Dr. Hare exhibited a species of ether, obtained by the reaction of alcohol, an acid, and a nitrite. This liquid differs from that heretofore known either as nitric, nitrous, or hyponitrous ether, in boiling at a lower temperature, in being much more volatile on exposure to the air, in having a more sugary taste, and in odour more resembling the liquid resulting from the reaction of chlorine with olifient gas. By evaporation it produced a temperature of 15° below zero of Fahrenheit. On contact with the tongue or finger it hisses as when a hot iron is introduced into water. By the process which Dr. Hare has adopted, the quantity of ether obtained is nearly equivalent to the quantity of absolute alcohol employed; whereas, according to Thenard, the process which he recommends as the best of those pursued in Europe, affords a quantity not exceeding a fifth of the alcohol.

It is anticipated that this new process will have a favourable influence both on the quantity, and price of the sweet spirits of nitre of pharmacy.

Dr. Hare has reason to believe that he has obtained a species of ether compounded of the elements of nitric and acetic ether, and which may deserve the appellation of nitro-acetic ether.

In the usual mode of conducting the process for sweet spirits of nitre, a most acrid principle has been detected which affects the nose like mustard or horse radish. This has been isolated and is supposed to be a chlorolganic ether.

In the ordinary processes for nitric ether, or sweet spirits of nitre, not only is much of the alcohol destroyed by the excess of oxygen, but the resulting products by uniting with the nitrous ether lessen its volatility and modify its qualities.

Besides the results above mentioned, Dr. Hare has procured an inflammable gas which seems to be an ingredient of the ether above described. Hence the new ether appears at first to boil at 50° Fahr., but the point of ebullition gradually rises to 65°, and yet after having thus risen, if the ebullition be suspended and afterward re-excited, it may take place at a lower temperature than was requisite, when the suspension took place.

This gas Dr. Hare finds to be permanent at the temperature of 6° below zero Fah. By great pressure it becomes a liquid resembling in colour the liquid ether. The effect of the gas upon the organs of smell and taste resembles that of the liquid ether, and it is conjectured that the gas and liquid contain the same elements, existing in the same ratio but occupying different volumes, or in other words they are metameric.

Bibliographical Notice.

"*Annals of Electricity, Magnetism and Chemistry, and Guardian of Experimental Science, Conducted by* William Sturgeon, *Lecturer on Experimental Philosophy, at the Honorable East India Company's Military Seminary, Addiscombe, &c., &c. And assisted by Gentlemen eminent in those departments of Philosophy.*"

The editor of this new Journal has been for several years known advantageously for his skill as an experimentalist, the ingenuity of his elucidations of theory, and the accuracy of his knowledge of electrical science in its varied and increasingly extensive applications in the phenomena of matter and motion. We have had quite a treat in looking over the first six numbers of "The Annals of Electricity" and have marked several extracts for republication in our own monthly. If sustained in a manner correspondent with the volume already published, it will vie in interest with any of its cotemporaries.

We are glad to find that the encouragement it has met with has induced the Editor, in order to furnish a vehicle more adequate to the abundant materials in his possession, to change it from a quarterly to a monthly publication, commencing on January 1st, 1838. In each of the numbers published there are at least two pages of lithographed plates, executed with uncommon neatness and clearness of outline, exhibiting an extensive variety of electrical, voltaic, and magnetic apparatus. The original communications are valuable, and the republication and embodying in one work the best essays and memoirs on this engrossing department ot knowledge cannot but facilitate an acquisiton which must be desirable to every student and teacher of science.

Progress of Practical and Theoretical Mechanics and Chemistry.

Articles from the French Journals; translated for the Journal of the Franklin Institute, by J. Griscom.

Pyrophorus of easy Preparation.

When two and a half parts by weight of pure tartaric acid, *deprived of its water of crystallization*, perfectly dry and reduced to powder, are mixed with eight parts of peroxide of lead in a dry capsule, it is well known that an ignition of the whole mass soon takes place, becomes very vivid. and lasts a long time. M. Böttigen has repeated this experiment with oxalic and citric acids, and finds that the action of the first on peroxide of lead is more prompt if not stronger than that of tartaric acid, and that the action of citric acid is a little weaker. By mixing five and a quarter parts by weight of peroxide of lead with one part of oxalic acid dried at a gentle heat (à l'air chaud) or containing 19 per cent. of water, an instantaneous ignition of the mass takes place, but it does not continue so long as with tartaric acid, because oxalic acid contains more carbon than tartaric. Citric acid must be kept sometime in previous fusion, then dried and pulverised, and one atom of it mixed with two atoms of the peroxide at a temperature of about 72°. The ignition of the mass is then as quick and continues as long as with tartaric acid. Minium, litharge and carbonate of lead also succeed with the latter, but less effectually than the peroxide. Jour. de Pharmacie.

Substitute for Soap.

M. Fenton, the inventor of this process, proceeds in the same manner as in soap making until the mass is ready for the moulds. He then dissolves common soda of commerce in water, and pours the solution on quick lime, taking equal quantities of soda and lime, and the mixture is boiled in a kettle. A quantity of alum equal to one half of the soda is well dissolved in water, and as soon as the contents of the kettle begin to boil, one part of the solution of alum and two parts of the mixture of the kettle are poured together into a separate vessel. This lye, at a proper temperature, will combine easily with soap. Receuil Industriel.

Separation of carbonic acid gas from sulphurous acid and sulphuretted hydrogen. By Gay Lussac.

It is necessary on a variety of occasions, to determine the exact proportions of a mixture of carbonic acid and sulphurous acid or sulphuretted hydrogen. I am aware that various methods have been proposed, and would scarely venture to say that the following is entirely new, and it may perhaps be worth recommending to chemists, only from the very simple manner in which I am in the habit of applying it.

After having measured the mixture in a graduated tube, I take a rod of glass a little longer than the measuring tube, and having smeared one end of it with flour paste and rolled the smeared part in pulverized oxide of manganese, which adheres to the paste, I insert it into the tube of gas, with the powdered end uppermost. Immediately the volume of gas rapidly diminishes, and in a few minutes the absorption of sulphurous acid or sulphuretted hydrogen is complete. If either of these two gases prevail in the mixture, or if there be too large a volume to be operated on, it may be advisable to withdraw the rod, wipe it clean, renew the paste and the manganese and repeat the operation. This however is but rarely necessary,—yet it is the less objectionable as the oxide is seldom detached from the glass rod and does not soil the inside of the tube. A band of wet paper may be wrapped round the rod instead of the paste, but it takes up less manganese.

The same mode of separation may be applied to the deutoxide of azote.

Ann. de Chimie.

Observation on the Assay of Silver in the moist way. By Gay Lussac.

In the course of the assays which I am daily making at the Warrant Office (bureau de garantie) Paris, I have had occasion to observe a new circumstance which may lead some into error who are employed in determing the value of plated materials in the moist way, if they are not aware of it. It arises from the fact, that sulphuret of silver is not very easily attacked by nitric acid; so that if the silver to be tried contains a few thousandths of sulphuret of silver, it may happen that this will not be dissolved and consequently too low an estimate will be given to the assay. This will take place, however, because the nitric acid is not strong enough or is not employed in sufficient quantity. Be it as it may, when the silver contains sulphurets, and a portion of it remains undissolved in the nitric solution, which will be known by the appearance of a very fine powder, heavy, of a dark colour, and different from the gold which silver sometimes contains in being less flocculent, the addition of a fresh quantity of concentrated nitric acid, will effect the solution of the sulphuret; but I have found it preferable to add to the solution of silver, when the presence of sulphuret is suspected, the amount of 5 or 6 cubic centimetres of concentrated sulphuric acid. This immediately dissolves the sulphuret; but to be more sure of it the nitric solution should be exposed for a few moments to the heat of boiling water.

The sulphuric acid must be free from muriatic acid. If it be not, it may be boiled for a short time, which will expel the muriatic acid; but I may say that I have never met with a sensible quantity of muriatic acid in the concentrated sulphuric acid of commerce. Ibid.

Thermometer Watch. By M. Jurgensen.

This is not larger than a common watch. The double arches of the balance wheel are externally of platina and internally of brass; the variation of its rate of going is about 32 seconds in 24 hours for every degree of the centigrade thermometer.

This instrument, placed in the open air like a common thermometer, will have its rate of going accelerated or retarded according to the atmospheric temperature. If then it be known to what degree of temperature the watch is regulated,—at what degree the needle beats exactly 86,400 times (answering to the number of seconds) in 24 hours, the difference between this number and the actual number of oscillations which it shall have made in twenty-four hours, will serve to show the *mean* temperature during that day, although the rate of going may have varied several times through the day. The observer then has only to compare its indications twice in twenty-four hours with a good clock or chronometer, to ascertain,the discrepancy, and convert it into degrees of the thermometer.

To render this instrument more generally useful, Mr. Jurgensen has added a metallic thermometer which gives the temperature at the moment of comparison, and by the aid of two hands the maximum and minimum during the twenty-four hours are also exhibited. Bulletin d'Encour. Mai.

Cordage of the fibre of Aloes.

According to M. Chevremont, Engineer of Mines, ropes made of aloes have four times the resistance of those of hemp of the same diameter and made by the same process. The fibres of the aloe contain a resinous substance which protects the ropes from the action of moisture even at sea, and renders the tarring of them unnecessary. They are lighter than hempen ropes, and lose nothing of their strength by being wet. When plunged into water even, they are shortened onlytwo per cent., so that they are much less stiff than hempen ropes. Ibid.

Culture of Beets in Lombardy.

There have been some remarkable peculiarities in the cultivation of the sugar beets in this country during the three years past. Its light and sandy soil suits them well, if the temperature which undergoes a sudden change on the first rains do not too long protract the sowing. Excessive droughts are also injurious, causing the portion of the root which is above ground to become green and acrid. Irrigations, so easily made in this country, and so favourable to rice, wheat and corn, injure beets, because they submerge the plants and prevent the unfolding of the leaves. To remedy these inconveniences, M. Pagen has advised the sowing of the seeds in beds and then transplanting the roots in raised beds enriched with good fine manure, immediately after the early rains. The roots will soon strike into this deep soil, and will be less exposed above ground. Water may be conveyed into the furrows between the beds without any risk of submerging the plants and the produce will be more certain.

Last year, the temperature and moisture being unusually favourable, beets were sown early, with the drill, and the gathering was abundant. The juice marked 5° on Baumé's acrometer, after being clarified. The raw sugar had a more agreeable taste, and the molasses was very sensibly less acrid than similar products in the north of France.

A large portion of land having been covered with the alluvial sands of the Adda, the beets sown upon it were unusually flourishing, but the juice was sulphurous and furnished but little sugar. Ibid.

Machine for cutting wood into thin leaves.

Mr. Picot has patented a machine by which extremely thin leaves, may be obtained either for cabinet work, or lithography. From an inch plank, he takes from twenty-four to thirty leaves, suitable for inlaying and as many as 170 adapted to lithography. The machine is very light, and is applicable to soft or hard wood, with an economy of one half compared with common sawings; there is no loss of wood. It can be worked by a single man, with the aid of a boy to gather the leaves as they are taken off. Its celerity is such that a thousand leaves may be cut in an hour from hard wood. Veneers from 28 to 30 inches long, and 14 to 16 wide may be cut by one or two men.

Ibid.

New theodolite, by M. Gambey.

M. Arago presented to the Academy of Sciences, a theodolite which M. Gambey had just executed, and which reflects both horizontally and vertically. Such are the neatness and regularity of the graduation; that, by the aid of verniers five seconds can be read off with the greatest certainty, on the two circles, though their radii are but three inches.

M. Arago remarks, that all the parts of this instrument are executed by mechanical means; and with respect to the division, there is this seeming paradox, that the graduating circle is not necessarily placed in the centre of the platform. M. Gambey has resolved that problem with great certainty, by connecting the traces or graduates with an articulated system of remarkable simplicity.

Ibid

Triangular Sweeper for cleaning streets.

This machine is composed of a triangular frame, mounted on three wheels and furnished with three score brooms. It is distinguished from others by the brooms being attached in rows of five each, to cast iron blocks, which play freely in grooved posts. Each group of brooms rises and falls according to the unevenness of the surface, and thereby always touches the pavement. As they wear out they fall by their own weight. If the machine is to operate in front, the dirt, collected at the point of the triangle, will be thrown to the right and left, without splashing, to the distance of eight feet, and is left in heaps. If the triangle works in the rear, the dirt of the gutter will be collected in the middle and brought to the sewer.

Ibid.

(N. B. The gutters in France are generally in the middle of the streets.)

G.

New process for extracting Sugar from Beets. By M. Schuzenbach of Carlsruhe.

This process consists in converting the beets into powder or dry flower, by a method which operates at once upon large masses and at little expense. The sugar is extracted from this farina, by a very small quantity of water, which comes out clear and limpid and so concentrated, that in order to obtain a given quantity of sugar, only two or three times its weight is necessary of this concentrated and limpid fluid. The subsequent manipulation, crystalization, &c. are thereby facilitated. The greatest quantity of crystalized matter contained in the beet, is thus also procured and but little molasses produced.

The author asserts that the expense by this method, is much less than by that commonly pursued, and that his apparatus occupies but little room.

Ibid.

Beet Sugar.

It appears to have been proved by M. Parrayon, beet sugar manufacturer at Mérignies, France, that by adding diluted sulphuric acid to the pulp, as much juice can be obtained by two cold pressures as by hot pressure, and that any injurious effect of the acid is completely removed by filtering the juice cold through animal charcoal properly prepared. It passes from the filter with an alkaline rather than an acid reaction. After being filtered, it undergoes clarification by the ordinary methods, requiring, however, rather less lime than usual. The sirup is remarkably white. One litre of it (2.1135 pints) produced 970 grammes (=31 oz troy,) whereas the same quantity, by measure, of the ordinary sirup, yields only 750 grammes.

Ibid. June, 1837.

Steam Navigation to America and India.

Sir,—Since my communication which you inserted in No. 733, upon the subject of steam navigation to India, I have been informed that the "Atalanta" sailed again from Bombay on special service, with 300 troops on board, on the next day but one after her arrival at that port; thus proving the excellence of her machinery, and the possibility of continuously working a steam engine for a considerable period without injury. It places the reasonings of Dr. Lardner on this point in a very questionable shape, and affords us humble mechanics something like a reasonable hope that the passage to New York may be accomplished in *one* trip, although Dr. Lardner has pronounced it to be next to impossible.

The steam ship called the "Great Western" has arrived in the river Thames for the purpose of receiving her engines. She was built at Bristol by Mr. Pattison, and measures about 1320 tons, old tonnage; when first launched she drew eight feet eight inches water abaft, and seven feet eight inches water forward; with 600 tons of coal on board, she draws eleven feet three inches water in the river Thames, or about eleven feet at sea, which shows that the displacement of the first two feet four inches above the light water line, is equal to 600 tons, and as she may be safely put down to fifteen feet water, the next four feet will displace, at least, 1200 tons more, making her total displacement 1800 tons at the least, which may be thus distributed: 500 tons for machinery, 800 tons for fuel, and the remaining 500 tons for additional stores, merchandise and passengers. She has now the greater part of her stores on board, as cables, anchors, sails, &c. which of course leaves nearly the whole 500 tons for merchandise and passengers.

She is fitting up in a very commodious manner for the convenience of passengers. The saloon will be very lofty and airy, and the state rooms (sleeping cabins) are large and convenient. The paddle or main beams are formed of four pieces of timber, each twelve or thirteen inches square, confined together at the ends, and separated to the distance of six or seven inches on the middle of their length on the same principle as the bow beams of Mr. Smart.

Her size, when seen by herself, does not appear so great as it really is, and it is only when on board, or seen alongside other vessels, whose size is known, that her magnitude is appreciated. She is nearly as long as the large steam vessel "Victoria," now building in the river Thames, but is not so wide, nor so deep as that ship.

The East Indian Government have purchased another steam ship, the

"Waterford," to send out to India. It is said that she is to be called the "Semiramis." It is a matter of regret, that a vessel expressly built for the conveyance of merchandise should have been purchased for this purpose;—it is not giving the trial of steam communication with India a fair chance. For, although well adapted for the purpose for which she was built, she is evidently not suitable for a steam frigate or a packet. One would have thought, that with the practical experience which the East India Company must possess, as to the rapid destruction of timber from the effects of climate, &c. in their Indian Possessions, they would have selected a vessel built with the most durable materials to send out, and not one in which red pine is so largely used as in the "Waterford."

Advices have been received from the "Berenice" (the last steamer dispatched by the Hon. East India Company) from which it appears that her progress has been more rapid than that of the "Atalanta" during the same part of the voyage. I am, Sir, yours, &c.

GEORGE BAYLEY.

London, September 11, 1837.

Mech. Mag.

Extraction of Silver Ore.

The study of the very gradual operations of nature has a bearing, the extent of which is far from being known, not more in a scientific, than in an economic point of view. The application which is about to be made of it in the extraction of some metals from their respective ores, particularly of silver, may here be noticed in illustration. The process most generally and longest employed in the management of minerals containing silver, consists in combining this metal with quicksilver, without the aid of heat, in a set of operations which has undergone few changes since the middle of the sixteenth century, the epoch of its discovery, and the theory of which is even now far from being well known. The amalgam is separated from the mineral by washing, and the silver from the mercury by heat. The amalgamation is effected in a viscous *magma* or lie, composed of the silvery mineral bruised into an impalpable powder, and likewise of mercury, common salt, sulphate of copper and of iron, of lime and of water, which are left to their mutual spontaneous action, whilst they are moreover frequently kneaded as it were under the hoofs of horse and mules. The different actions which are produced in this magma have been carefully examined, since those principles which were formerly neglected have begun to be studied. Since that time, the different causes which are sometimes opposed to the chloridation of the silver, and to the decomposition of the chloride by the mercury, whence losses sometimes result in the process, have been discovered. By removing these disturbing causes, and by reducing the problem, so to speak, to its most simple expression, silver has been extracted from a great number of ores, without the employment either of heat or of quicksilver, by employing sea-salt only, and some chemical products which are very easily procured every where, and iron, or iron and silver only in such a way that silver is extracted by the silver. All that is required, then, for the working of these valuable minerals is, that the localities whence they are procured be found in the neighbourhood of the sea, or of those great collections of rock-salt which are concealed in many parts of the world.

The first experiments were made in tubes which were almost capillary and upon the most minute fractions of the ore. The results have been such, that after a few trials, experiments have successfully been made upon many hundreds of pounds. And it is now hoped that in a very short time, art will be able to avail itself of a process which, in addition to the pecuniary advantages it affords, will enable us to reduce those valuable ores, which now cannot be subjected either to amalgamation, or to the action of fire, more especially those which contain copper in great quantaties, and whose management has always been an impossibility to metallurgists.

The importance of working minerals containing silver, without the aid of mercury, will be easily appreciated, when it is known that the registers of the mint of Potosi bear witness that between 285 and 286 millions of *marcs* of silver have been struck from the year 1570 to the year 1800, and that in the preparation of this enormous sum 286 millions of pounds of mercury have been lost, which, at the present price, represent a capital of L. 62,500,000. This immense quantity of quicksilver, used at Potosi alone, is now in the bed of the Pilcomayor, a river of Peru, into which flows all the refuse and rubbish of the siver mine. What a mine of mercury is this same, so soon as art shall have devised means for its easy and economical recovery.—*Becquerel on the relations of Natural Philosophy uith Chemistry and the Natural Sciences.*

Changes of the Zero Point in a Thermometer. By M. C. Despretz.

It appears to have been satisfactorily proved by C. Despretz, that the freezing point of a mercurial thermometer, owing to changes in the structure of the glass, is constantly liable to fluctuation. Its tendency is to ascend on the scale. In experiments with two thermometers, often repeated at intervals between the 30th of August 1832, and the 23d of June 1837, the zero point of the centigrade scale rose in the one case from 0° to 47°, and in the other from 0° to 45°. Their sensibility was such that one hundredth of a degree could be estimated in one, and from two to three-hundredths in the other. This movement of the zero point is produced both by sudden heating and cooling and by the gradual influence of time. If the temperature of an instrument is kept very low, the freezing point ascends; if elevated, it sinks; and this change may be perceived even on suddenly cooling and heating the instrument. Thus, if the freezing point of water be taken, then the boiling point, and then again the freezing point, the latter will be found depressed below its former mark, by a quantity or space which ought not to be neglected in nice experiments, for it sometimes amounts to a third of a degree even in thermometers with a small reservoir.

Whenever, therefore, the determination of fixed temperatures is an object of experiment, the position of the zero ought to be previously ascertained; but if the object is only an estimation of differences of temperatures the oscillations of the zero may be disregarded, since the value of each thermometric degree depends only on the mass of the quicksilver (which remains absolutely the same) and on the co-efficient of the dilatation of the glass which cannot have varied by an appreciable quantity. In these cases we may dispense with taking the zero.

Science presents facts which appear somewhat analogous to the changes in glass now alluded to. Pietet shewed in 1816 that a bar of iron about an inch in diameter and a little more than 100 inches in length, pressed or strained by a weight of 260 pounds, did not return exactly to its primitive length when the weight was removed; and that this was also the case when it was heated or cooled, the bar always remaining too long or too short after returning to the primitive temperature. The fact of the rupture of a bar, under a weaker load than that it had before resisted, is of the same kind. Various observations of M. Savart on torsion may be referred to the same principle, as well as another fact alluded to by him, viz. that a roll of sulphur gives sounds more and more sharp, for a certain length of time. Do not all these facts concur to prove that every time the molecules of a solid body undergo a displacement by any mechanical cause, as pressure, attraction, or torsion,—or by a physical cause, as an elevation or depression of temperature, they do not resume exactly their primitive positions when the causes have been removed. Annales de Chemies.

The changes in the interior arrangements of the molecules of a body, brought into view by the forgoing extract, are not without an application to the usual mode of trying the strength of steam boilers, the process commonly resorted to being mechanical, and not by the agency of heats. Tr.

Gas Coke as a Fuel for Melting Iron.

The following communication possesses much interest for all concerned in the manufacture or use of iron casting. In the preparation of this material, time is emphatically money, and the experiments at the Franklin Works show that a saving may be effected of one-half the time usually required in melting. Com Pub.

Franklin Works.
Philadelphia, January 1, 1838.

Dear Sir,—At the suggestion of Mr. Cresson, Superintendant of the Gas Works, we made, some weeks since, a few trials of gas coke as a fuel for melting iron. They were not made with reference to exact results because having never heard of gas coke being used in England for this purpose, we doubted the success of the experiments.

A few days, however, convinced us that the coke presented advantages over the anthracite which would render it an important fuel for iron founders, and we determined upon making some exact comparative experiments upon the use of the two fuels. The results of which we deem it proper to send you with liberty to make such use of them as the Board may deem expedient.

Our cupola is thirty inches in diameter. The blast is urged into it by a fan with four wings having an agregate area of 384 inches of fan surface moving 1800 revolutions per minute through three tuyeres, two of them 4½ inches in diameter, and one of 5 inches. The aggregate area being fifty-one inches and a half.

The trial was made in two heats with each kind of fuel and the results given are the aggregate of both heats respectively.

1st. *Anthracite Coal*, white ash of very excellent quality,

Time of blowing 1st heat	3 hours 15′
2d "	3 " 30
aggregate	6hs. 45′ or 405 minutes.
Metal melted	15.464 lbs. or 2300 lbs. per hour.
Fuel used	2.300 " or 470 lbs. to the ton of iron.

2d. *Coke.* Time of blowing 1st heat 1hr. 54′
2d " 1 18′
aggregate 3 12′ or 192 minutes.
Metal melted 14.342 lbs. or 4450 lbs. each hour,
Coke used 3.056 " or 470 " to the ton of iron.

The weight of fuel used to each ton of iron melted, was in both cases precisely the same, but the quantity melted in the same time was nearly double. The importance of time in a shop where many hands are employed, can readily be appreciated and would of itself be a sufficient inducement to ensure it a preference even at an advanced cost.

The capacity of a cupola for large castings is increased in proportion to the increased rapidity of melting—while with the anthracite our operations were confined to castings weighing not more than from 30 to 3500; we should have no hesitation of undertaking a piece of work weighing from three to four tons from our cupola using coke as fuel.

Merrick & Agnew, Iron Founders, Southwark.

Dr. R. M. Huston, *President Philada. Gas Works.*

Gowland's Artificial Horizon.

Sir,—At the late meeting of the British Association, in Liverpool, Mr. William Ettrick gave a description of several artificial horizons; among others, of one invented and constructed by himself. This instrument consisted of a black glass plane, with a pendulous rod attached to the under surface, and was supported by centres playing in gimbals. The rod had a weight affixed to its lower end, for the purpose of maintaining the horizontal position of the plane.

Under these circumstances I deem it right to state, that in the early part of the year 1835, Mr. Gowland, chronometer maker, of Leathersellers Buildings, London-wall (the ingenious inventor of the triple-pointed steel pens, &c.,) showed me an artificial horizon which he had introduced, consisting of a flat plate of opaque black glass, which floated upon the surface of mercury contained within a circular box, somewhat resembling a large tobacco-box. The glass was of a very beautiful jet black colour, and was levelled with extreme care, the positive efficiency of the instrument mainly depending upon the accurate parallelism of the two faces. Suspended by centres, moving in gimbals, with a weightier rod screwed to the bottom of the box, this horizon, would be adapted to use on ship-board. For the purpose of land-service, however, the plumb-rod is unnecessary, the instrument being complete in its original simple and compact form. The portability of Mr. Gowland's artificial horizon, with the circumstance of its requiring no verification or adjustment previous to using, sufficiently accounts for the high value that has been attached to this instrument, and justifies the preference it has attained over some other inventions of a similar nature.

Extraordinary care must of necessity be used in affixing the pendulous rod to Mr. Ettrick's glass plane, so as to obtain a true horizon, and I apprehend it must be liable to derangement in use. In Mr. Gowland's apparatus no such nicety is required; *self-adjustment* always takes place.

The tremulous motion of the mercury, which is so decidedly objectionable to its use for a reflecting surface, is nullified by the superincumbent plane of glass, while the true horizon is always obtained without trouble, and with great certainty.

William Baddeley.

Wellington street, Blackfriars road, Sept. 26th, 1837.

Mech. Mag.

Progress of Physical Science.

Optical Illusions.

If we consider for a moment, in what way we may distinguish a concave from a convex surface, we shall find that it merely consists in a different distribution of light and shade; those parts which are light in the one, being shaded in the other, and *vice versâ*; the deception produced in objects of this form, by inverting the position of the image, is most striking, and affords one of the best instances of the fallacy of our reasoning, when we lose one of the data on which we ground our inferences.

If we take any bright convex or concave surface, no matter what the material, and place it between the window and the eye, so that the eye sees it by looking *downwards* at an angle of from 20° to 60°, there is a certain distribution of light and shade on the object, which enables the eye, from habit, to determine whether it be convex or concave; if we can now invert the position of the edges of the object, with reference to the eye and the window, the shaded parts assume just the position which would be assumed by the opposite curvature; now, this inversion may be done without any lenses whatever.

Take a common Wedgwood-ware evaporating-dish, or a common tea-saucer, a basin, or any object which is concave on one side and convex on the other—a watch-glass even will do—and place it on a table between the observer and the window, with the concave side upwards, then hold a piece of common glass (with the under side blackened) in such a position that an image of the object will be reflected from its surface to the eye: the laws of optical reflection teach us that the rays from the farther edge and those from the nearer edge cross each other in their passage to the eye, and assume an inverted position, it will now be seen that the object has every appearance of being a convex surface, and if the angle of observation be well chosen, (which depends partly on the height of the window,) the illusion is so strong that it can scarcely be removed from the mind. If a convex mirror be substituted, and its position well adjusted, the distortion will present all the appearance of a concave mirror.

It is not essential to this experiment that the reflector should be black on its under surface, although it is more favourable for the purpose; any polished surface will suffice, provided it receives the rays at a large angle of incidence: it can be seen very well by reflection from one of the faces of a prism, and a singular effect is produced by a particular adjustment of the prism, by which two images of the same object may be conveyed to the eye within a short distance of each other; one is the regular refracted and transmitted image, and the other a reflexion from one of the *internal* faces of the prism: one of these images will be convex, and the other concave, and their juxtaposition forms a very remarkable appearance—one being prismatically coloured and *convex*, and the other of its natural colour and *concave*.

If the student in optics carefully considers the law that "the angles of incidence and reflection are always equal," and will carry out that principle to it fullest extent, he will find that every one of the phenomena of which we have been treating depends solely on the operation of that law; if he considers the position of the source of illumination, the degree of curvature of the surface on which any ray falls, and the angle at which the eye receives the reflection, he can clearly show that that law determines what part of the object will appear bright and what part shaded.

The same law determines the production of another phenomenon, as remarkable and much more beautiful than the former; if we take a plate of looking-glass, and sprinkle a few grains of dust upon it, and view it perpendicularly with one eye at a few inches distance, the dust appears arranged in a most beautiful radiating star; of which the eye is the centre; it matters not how unequally it is scattered; the symmetry is invariable.

Now in such a case as this, before we begin to *theorize*, it is proper to extend the experiment to other objects, and in accordance with this, it will be found that if the object be a polished surface of steel, of silver, of mercury, or any substance but silvered glass, no such radiating appearance presents itself; if now the student reverts to the law above alluded to, he will at once detect the cause; when the dust is scattered on the surface of silvered glass, an image of each little particle is reflected from the *mercurial surface* through the glass again to the eye; as the angle at which we view any particle increases, so must the apparent distance between it and its image increase, until we arrive at 45°, which gives the maximum of distance.

Now the particle and its reflected image are in the same vertical plane which passes through the eye and the image of the eye; the particle, its image, and the image of the pupil of the eye appear, therefore, to be in the same right line, and as the same applies to every particle, as the line which joins the particle and its image would pass, if produced, through the image of the pupil of the eye, wherever the particle be placed, the pupil appears to be a nucleus or centre from which all these double images extend radially outwards.

When, however, a polished metal is used, there is no reflecting surface from which a *second* image can be obtained, and we therefore see the particles themselves, unaccompanied by any reflected image, and the symmetrical appearance is not then produced. The employment of a concave mirror affords an excellent proof that this beautiful radiating appearance is the effect of reflection. If we hold the eye at the focus of the mirror—that is, at the centre of the sphere of which the mirror is a section,—we see none of these radial lines, but as we approach near to the mirror, they gradually develop themselves; this arises from the circumstance, that when the eye is at the centre of the sphericity, incidence and reflection are both perpendicular to the mirror, or rather, the reflected image is concealed by the particle itself, and therefore cannot reach the eye; but when we approach nearer, the images of the *lateral* particles becomes visible, and the starry effect begins to appear.

In a *convex* mirror, the effect is very beautiful, on account of the increased obliquity which its curvature gives to the incident ray, and the non-existence of a focal point in front of the mirror.

The effect is greatly heightened by mixing powders of two different colours—red and green, or blue and orange, for instance,—as those colours which are complementary to each other produce a singularly pleasing effect by their juxtaposition. A very slight sprinkling of milk, or any coloured liquid, likewise produces this symmetrical effect.

Instead of using glass, we may use two fluids of different densities and different refractive powers, but the two which best answer the purpose are water and mercury; if a little powder be sprinkled on the water, it will be reflected from the mercury under the water, and the same effect produced as with glass, but the latter is the more convenient of the two.

If the eye be directed to one *corner* of the glass, the particles will have the appearance of an expanded fan, emanating from that corner.

Thus may the same simple law be brought to bear upon the whole of the appearances which we have now detailed: in all these instances an object, or an assemblage of objects, *appear* to be what they are not, simply from our neglecting to seek for an explanation from the only sources to which we should apply in such cases, viz. the unerring laws of Nature, the simplicity of which is as conspicuous as their beauty and their universal application.
T.

Mag. Pop. Sci.

A New Anemometer.

At the late meeting of the British Association at Liverpool, Mr. Osler, of Birmingham, read an account of a new Registering Anemometer and Rain Gauge, now in use at the Philosophical Institution at Birmingham, illustrated by diagrams, giving a condensed view of the observations recorded during the first eight months in the year 1837.

In this instrument the direction of the wind is obtained by means of the vane attached to the rod, or rather tube that carries it, and consequently causes the latter to move with itself. At the lower extremity of this tube is a small pinion working in a rack, which slides backwards and forwards as the wind moves the vane, and to this rack a pencil is attached, which marks the direction of the wind on a paper ruled with the cardinal points, and so adjusted as to progress at the rate of one inch per hour by means of a clock. The force is at the same time ascertained by a plate one foot square, placed at right angles to the vane, supported by two light bars running on friction rollers, and communicating with a spiral spring in such a way that the plate cannot be effected by the wind's pressure, without constantly acting on this spring, and communicating the quantum of its action by a light wire, passing down the centre of the tube to another pencil, below which it thus registers its degree of force. The rain is registered at the same time by its weight acting on a balance, which moves in proportion to the quantity falling, and has also a pencil attached to it recording the result. The receiver is so arranged as to discharge every quarter of an inch that falls, when the pencil again stands at zero.

Mr. Whewell spoke highly of the construction of this anemometer; he had no doubt but that a very slight modification of the mode of registering its indications would cause it to answer every purpose which he had lately described as desirable. In its present form, however, it was the force of the aërial current, which it indicated, not the integral effect. He also highly commended the rain-guage, and the method of showing in one diagram to the eye so many important meteorological phenomena. Professor Lloyd stated, there was a very simple method of causing the anemometer of Mr. Osler to give the integral effect of the wind, and that was to cut out the paper covered by the tracings of the pencil indicating the force of the wind, and to weigh it; for it was easy to perceive, that since the ordinates of the curved spaces covered by those tracings were proportional to the force, and, therefore, the velocity of the wind, and the abscissæ to the time, the areas represented the integrals, or the total amount of the aërial current.—Mr. Ettrick asked, whether some other method of supporting the cylinder which moved backward and forward as the force of the wind varied, rather than friction rollers, would not be desirable—such, for instance, as bridle rods, or other means known to practical mechanics, and he was sure, well known to Mr. Osler. Mr. Osler replied, that many methods of supporting this part of the apparatus had been tried and laid aside, as not answering; among the rest, bridle rods. Mech. Mag.

Simultaneous Meteorology. No. III.

It is earnestly to be hoped that the periodical Hourly Meteorological Observations, suggested by Sir John Herschel, may be universally made, and that with the most scrupulous fidelity and accuracy. An abundant harvest of precious facts will soon present itself to students of meteorology.

One instance of the innumerable and deeply-interesting comparisons, which may be made by means of these series, has been furnished us by a correspondent.

The tables annexed have been drawn up from observations which were made at the March-Equinox of this year, and since published in this or other scientific journals. They contain the more remarkable meteorological circumstances which occurred at two places in different quarters of the world, lying on different sides of the equator, and about six thousand miles asunder; viz., Blackheath in Europe, and Feldhausen in Africa. They also exhibit the atmospheric phenomena which existed at the same instant of time in these two widely-distant spots. When this shall be done in every region of the earth and sea, meteorology will possess a million-eyed Argus, whose watchings will four times every year, detect and expose the similitudes or differences in the physical condition of the atmosphere of a world.

We abstain from indicating results, and pursuing reflections, which an examination of these tables naturally elicits and suggests, because, at present, the meteorological reports do not all of them posses sufficient data for a satisfactory comparison, and have not all been subjected to those processes of reduction which are necessary for such a purpose. In the present case, for example, the European observations of the barometer are not reduced to 32° Fahr., as are the African ones; nor is the difference of level of the two stations given, &c. Still they are sufficient to show that, at the period in question, the pressure at the two places was almost identically the same,—that the temperature was extremely different,—that its range was remarkably greater at Feldhausen than at Blackheath, &c.

No. I.

TABLE of the maximum and minimum Pressure and Temperature of the Atmosphere during the Hourly Series of Observations for the March-Equinox, 1837, made by Mr. J. H. Belville, at Blackheath Road, near Greenwich; to which are annexed the Pressure and Temperature at the same instants of time at Feldhausen, near Wynberg, Cape of Good Hope.

BAROMETER.

Blackheath Road. Feldhausen.

	Time.		Corresponding Instant.			Diff. at the same Inst.	Place where the mer. col. was highest.
in.	h. m.				9 in.	in.	
Max. 39·971	VI. 0 A. M.	21 Mar.	VII. 13½ A. M.	21 Mar	29. 74	0.247	Blackheath Rd.
Min. 29.796	IV. 0 A. M.	22 —	V. 13½ A. M	22 —	29 .72	0.004	Blackheath Rd.
	V. 0 A. M.	22 —	VI. 13½ A. M.	22 —	29 .82	0·006	Feldhausen.
Diff. 0.175							

THERMOMETER.

Blackheath Road. Feldhausen.

		Time.		Corresponding Instant.			Diff. at the same Inst.	Place where the mer. col. was highest.
Interior.	°	h. m.				°	°	
	Max. 43.0	XI. 0 P. M.	21 Mar	O. 13½ A. M.	22 Mar	70.0	27.0	Feldhausen.
		XII. 0 P. M.		I. 13½ A. M.	22 —	70.0	27.0	Feldhausen.
		I. 0 A. M.	22 Mar	II 13½ A. M.	22 —	70.0	27.0	Feldhausen.
		II. 0 A. M.		III. 13½ A. M.	22 —	70.2	26.8	Feldhausen.
		III. 0 A. M.		IV. 13½ A. M.	22 —	70.2	26.8	Feldhausen.
		IV. 0 A. M.		V. 13½ A. M.	22 —	70.0	27.0	Feldhausen.
		I. 0 P. M.		II. 13½ P. M.	32 —	70.0	27.0	Feldhausen.
		II. 0 P. M.		III. 13½ P. M	22 —	70.0	27.0	Feldhausen.
		III. 0 P. M.		IV. 13½ P. M.	22 —	70.0	27.0	Feldhausen.
	Min. 35.0	X. 0 A. M.	21 Mar	XI. 13½ A. M.	31 —	70.0	35.0	Feldhausen.
		XI. 0 A. M.		O. 13½ P. M.	21 —	70.	35.0	Feldhauson.
	Diff. 8.0							
Exterior.	Max. 38.4	XII. 0 NOON	22 Mar	I. 13½ P. M.	22 —	75.8	37.4	Feldhausen.
	Min. 24.2	VI. 0 A. M.	21 Mar	VII. 13½ A. M.	21 —	67.0	42.8	Feldhausen.
	Diff. 14.2							

No. II.

TABLE of the maximum and minimum Pressure and Temperature of the Atmosphere during the Hourly Series of Observations for the March-Equinox, 1837, made by Sir JOHN HERSCHEL, at Feldhausen, near Wynberg, Cape of Good Hope; to which are annexed the Pressure and Temperature at the same instants of time at Mr. BELVILLE's Meteorological Observatory, Blackheath Road, near Greenwich.

BAROMETER.

FELDHAUSEN. BLACKHEATH ROAD.

		Time.		Corresponding Instant.			Diff. at the same Inst.	Place where the mer. col. was highest.
	in.	h. m.		h. m.		in.	in.	
Max.	29.844	IX. 0 A. M.	22 Mar	VII. 46½ A. M.	22 Mar	29.836	0.008	Feldhausen.
Min.	29.705	I. 0 P. M.	21 Mar	XI. 46½ A. M.	21 Mar	29.892	0.187	Blackheath Rd.
Diff.	0.139							

THERMOMETER.

FELDHAUSEN. BLACKHEATH ROAD.

			Time.		Corresponding Instant.			Diff. at the same Inst.	Place where the mer. col. was highest.
		°	h. m.		h. m.		°	°	
Interior.	Max.	71.0	II. 0 P. M.	21 Mar	O. 46½ P. M.	21 Mar	36.0	35.0	Feldhausen.
	Min.	69.0	VIII. 0 P. M.	21 Mar	VI. 45½ P. M.	21 Mar	38.0	31.0	Feldhausen.
	Diff.	2.0							
Exterior.	DRY.								
	Max.	92.5	II. 0 P. M.	21 Mar	O. 46½ P. M.	21 Mar	31.1	61.4	Feldhausen.
	Min.	54.5	I. 0 A. M.	22 Mar	XI. 46½ P. M.	21 Mar	28.7	25.8	Feldhausen.
	Diff.	38.0							
	WET.								
	Max.	65.6	VIII. 0 A. M.	21 Mar	VI. 46½ A. M.	21 Mar	not	ob-served.	Feldhausen.
	Min.	51.3	I. 0 A. M.	22 Mar	XI. 46½ P. M.	21 Mar			Feldbausen.
	Diff	1.47							

Mag. Pop. Sci.

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*On the Yellow Colour of leaves in Autumn, by Berzelius.**

Various researches have been made upon the colours which the Autumnal foliage assumes, after a few frosty nights. Macaire Princeps concludes from his experiments that the leaves having ceased to disengage oxygen, absorb this gas from the air and form an acid, which changes their colour first to yellow and then to red, and that this acid can be neutralized by an alkali and the green colour restored. He regards these changes as modifications of one and the same colouring matter which he calls *chromule*, and asserts that it is the cause of the common yellow or red colour of the petals. These results are very erroneous. A leaf does not become green by any reagent; but when the foliage has become red, potash will change it to green, because the red colouring matter forms green compounds with that alkali.

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*Translated for this Journal by J. Griscom.

Berzelius experiments chiefly upon the leaves of the Pyrus communis gathered in a fresh state, after the colour had turned to a citron yellow. The leaves were maserated in successive portions of alcohol, and also treated with boiling alcohol. The latter became gelatinous on cooling. A granular substance was thus obtained, which had a tendency to crystallization, and also a yellow, soft, fatty substance which appeared identical with the grains. These contained the yellow colouring matter of the leaves, which is described as a yellow, fatty, unctuous substance, easily melted, and then on cooling becoming concrete, transparent, of a brownish yellow, decomposable by heat, and leaving a residuum of charcoal. When moistened with water and long exposed to air and light, it loses its colour completely and is transformed into a greasy matter, which is regarded by Berzelius as a peculiar substance, intermediate between fat oils and resins. He names it *Xanthophylle* (yellow leaves,) and is of opinion that the transformation of the green colouring matter of the leaf into a yellow, is effected by a change in the organization of the leaf produced by frost which modifies the organization. Every effort to reproduce the green from the yellow proved fruitless, nor could he succeed in changing the colouring matter to yellow. The brown colour of the foliage has nothing in common with the yellow. It is produced by an extractive principle first colourless, and which, after the disorganization of the epidemics of the leaf, becomes brown by the action of oxygen, and communicates to the fibrous skeleton of the leaf a brown colour which cannot be removed by a weak solution of caustic potash, or destroyed by sulphurated hydrogen.

The colouring matter of red leaves, Berzelius considers as probably identical with that of red fruits. He examined more particularly that of the leaves of the cherry and of the red gooseberry. It was extracted from the leaves by alcohol, after distillation was separated by the filtre from a resin and a fatty precipitate. The red matter after filtration was thrown down by neutral acetate of lead, of a beautiful grass green, which soon become a greyish brown.

The red colouring matter of the leaves, he names Erythrophylle (red foliage.) When precipitated in parts by lime water, it subsides of green colour while the fluid a paler red tint,—hence this colouring matter is not originally blue. Jour. de Pharmacie Juilliet 183 .

Progress of Civil Engineering.

Construction of Railways.

The Llanelly Railway and Dock Company are adopting a new mode of constructing the upper works of railways, under a patent granted to Mr. Thomas Parkin, who has delivered several lectures upon the subject in London.

As the subject of railways is generally interesting, and must be especially so to the inhabitants of this part of South Wales, which, through the advancement in science, seems destined to become, at no distant period, a very important seat for the manufacture of iron, we shall not hesitate to give a particular description of Mr. Parkin's plan. The road, being prepared of longitudinal bearers of *Kyanized* oak, or other hard wood, from four to six inches in the base, two at the top, or four or five feet deep, dovetailed into one another, thus forming a continuous line, with uprights, twelve inches long and about three in diameter, screwed firmly at every yard into their bottom, are fixed, at a proper height and level, in trenches dug in the road, from eighteen inches to two feet wide and deep. The trenches are then filled with concrete, composed of gravel, sand, and Aberthaw lime, in proper proportions, when only the top of the bearers is visible on the surface of the road, all the rest being completely buried in and consolidated with the concrete, which immediately sets and hardens, and in a short time becomes a solid rock, the whole length of the road forming, as an engineer who inspected the work said, "an everlasting railway." The foundation being complete, patent felt is laid upon the top of the bearers whereto bars of iron, two inches wide, and half an inch thick, are firmly screwed, and the road is finished. The dimensions of the trenches must be regulated by the nature of the soil on which the railway is made, while the parties adopting the plan may use wood and iron of any size they think proper. The rails, as at present fixed in iron chairs, mounted on stone blocks, at every yard, form unyielding points of support, and intermediate flexible spaces. The carriages, of necessity, alternately descend below, and rise above, the mean line of progression, and produce concussions on those points, the severity of which is according to the degree of variation from a straight line and the *rapidity* of the motion. At a speed of twenty-five miles per hour, they would occur 733 times in a minute. On the Manchester and Liverpool line, Professor Barlow "found the whole road to tremble;" and the deflection in the rails so great as materially to diminish their strength, in consequence of their being *permanently* fixed to the chairs.

Upon Mr. Parkin's plan, no vibration, of the *least possible* degree, is felt, the bars being screwed to *continuous* wooden bearers, bedded upon an unyielding mass of concrete, of the strength and durability of which cement the Roman roads are irrefragible evidence. Besides, the patent felt, between the iron and the wood is a non-conductor of vibration, and aids in lessening the tremulous motion attendant on carriages running even on a perfect plane. The stone blocks in the ordinary mode of construction, being insulated and independent of each other, the preservation of a level surface (even if its attainment were practicable) and of parrallelism in the rails, is impossible, as differences in the subsoil must occur; and when a block has sunk, in ever so trifling a degree, below those next it, the next will either remain in the permanent flexure, or be loosened from the chair, or loosen the chair from the block, or hold the block in suspension. The rails and chairs form a series of levers and fulcra, the action and re-action of which are incompatible with permanent stability.

Upon Mr. Parkin's plan, an even surface and parrallel lines may be had, with almost mathematical exactitude, which neither the magnitude of the weights passing on the rails, nor the rapidity of their motion, can disturb, after the concrete has acquired a proper degree of hardness. When a block on one side of an ordinary railway has sunk only in a slight degree, while the corresponding one opposite obtains its position, increased weight will, of necessity, be thrown on the former, and its tendency to sink still further as well as to be forced out of a parallel line be augmented, in the ratio of the sinking. But with Mr. Parkin's rails no such sinking could be anticipated, even were its occurrence possible.

With level parallel rails, scarcely any lateral pressure can be felt, except in curves, where means will be employed to meet the exigency of the case. The absence of a plane and of parallelism in the rails, is the principal, if not the only, cause of lateral pressure, which could do no injury to Mr. Parkin's road, unless it either removed out of its place one-half of the mass of concrete in which the bearers, with uprights a foot long at every yard, are buried—made a breach in the continuous line of bearers—forced the strong iron screws, by which the bars are secured to the bearers, from their hold, or burst the bars themselves asunder. As these bars are only half an inch wide, and are thus secured to continuous bearers, they present no leverage for the flanch of the wheel to act on, when any lateral pressure takes place; whereas in ordinary railways, the leverage is considerable (the wheels being elevated the height of the rail, which is four or five inches above the point of support,) and produces a corresponding tendency to force the blocks out of a straight line. Upon such an unyielding plane as Mr. Parkin's railway presents, less tractive power, as Professor Barlow has ably demonstrated, is necessary than on rails which deflect between the points of support, while the wear and tear of carriages and engines, and the necessity for limiting their weight, are materially lessened.

A locomotive engine weighs from ten to twelve tons, and subjects the rails to the process of a species of rolling, when they split, and become only of the value of old iron. To such an extent was this the case on the Manchester line, that after five or six years it became necessary to have new ones. The less the vibration and concussion, the less liable are the rails to receive this injury, especially when supported on the softer substances of felt and wood. Moreover, as Mr. Parkin's bars only weigh ten pounds per yard, instead of sixty or seventy, the loss from such injury will be proportionably less. Mr. Parkin's plan may be executed cheaper, even where stone blocks cost little, than any other, and with astonishing rapidity. In most parts the saving, by its adoption, will be fully one half. It may be applied to colleries and private works, especially in Wales, on very advantageous terms. We understand the plan has been recommended by Mr. Brunel for adoption by the directors of the Great Western Railway.—*Cambrian.*

Mining Journal.

Consumption of Coal in Great Britain.

The quantity of iron annually produced in Great Britain may be taken at 700,000 tons; and the quantity of coal required, at an average, to produce each ton of iron, including that used by engines, &c. may be estimated at 5½ tons; giving a total of 3,860,000 tons consumed in the making of iron. According to Mr. Kennedy, the quantity of coal consumed in the cotton manufacture, in 1817, was upwards of 500,000 tons, and the manufacture has since more than doubled; so that, allowing for greater economy, we may fairly estimate the consumption of coal in the cotton trade at 800,000 tons a year. Its consumption in the woollen, linen, and silk trades cannot be less than 500,000 tons. The smelting of copper ores of Cornwall consume annually about 250,000 or 300,000 tons; and it is supposed that the brass and copper manufactures require nearly as much. In the salt works of Cheshire, Worcestershire, &c. the consumption is probably not under, if it do not exceed, 300,000 tons. The consumption in lime works may, it is believed, be estimated at 500,000 tons. It would appear, therefore, that the total annual consumption of coal in Great Britain may be moderately estimated as follows:—

	Tons.
Domestic consumption, and smaller manufactures	15,000,000
Production of pig and bar iron	3,850,000
Cotton manufacture	800,000
Woollen, linen, silk, &c.	500,000
Copper smelting, brass manufactures, &c.	450,000
Salt-works	300,000
Lime-works	500,000
	21,350,000
Exports to Ireland	750,000
Ditto to colonies and foreign parts	600,000
Total	22,700,000

If we suppose that the above sum of 22,700,000 tons costs the consumer, on an average, 7s. a ton, it will be worth, in all, £7,955,000 a year!—*M'Culloch's account of the British Empire.* Mech. Mag.

Deakin's Improved Blast Furnace for Smelting Iron Ore, &c.

In the pretent method of filling blast furnaces, the whole of the materials for making iron, namely, coal or coke, mine, limestone, &c., are put in at the top of the furnace. On this improved plan, the mine and limestone, will, at first, still be put in there; but when the furnace works in a certain state, a part of the mine will be put into it at the flues; and also the greater part of the fuel used for smelting the iron. If pipes are used they may be carried up on the outside of the stack of the furnaces, and may be applied to any blast furnace now in work at a small expense. When this plan is adopted on a furnace being repaired, or newly built, the chimney flue should be carried up in the furnace stack. The coal, or coke being put into the furnace mixed with limestone, mine, scroffula, and rubbish, however free, such coal or coke may itself be, when put in, at present, it must become impregnated with the filth that accompanies it, as soon as the heat begins to act; and it is, moreover, consumed before it comes down to the boshes, and consequently, the power of the fuel is destroyed, before it arrives at the part of the furnace where it is wanted to smelt the mine; but by putting the fuel into the furnace, it will meet the blast in the proper place, free from impurities, and in full strength to melt the materials above it. Better iron will thus be made, with a smaller quantity of fuel, than is done by the present method of filling blast furnaces. Ibid.

Mechanics' Register.

Hot Blast.

"A Founder" in the London Mining Journal objects to the "Patent Hot Blast" that the pig-iron is sadly deficient in point of strength, compared with pig-iron made in the *good old way;* that a cupola man can (seemingly with ease) take up a No. 1 "Patent Hot Blast" foundry pig, and break it across his knee; that in charging for a casting of five tons weight, for instance, we find it necessary to charge with six tons, whereas five tons twelve and a half cwt. of cold blast pig used to be quite sufficient; that all the founders have been obliged to increase the size of all patterns for castings that are likely to be subjected to much torsion or friction, since the use of the patent has become general in this country. That it has been a valuable discovery to the patentees and iron masters can never be doubted. But it still remains to be shown where and how the public have been benefitted by the invention. And he enquires what has become of the *obdurate toughness* of the genuine Scottish cold-blast pigs.

Another correspondent states that the pig iron obtained from the use of anthracite coal, by Crane's process, is not at all weakened by hot blast, but is better calculated for the founder's use than any he had seen,—that beams cast from this iron of the same pattern with others from coke iron No. 1 had twenty per cent. the advantage. The reason, he says, why pig-iron smelted with anthracite is not weakened by the hot blast is the great quantity of carbon it contains. It is put into the furnace as it comes from the mine (not being cokeable) and being of a ponderous and compact nature, the cold blast could not penetrate as in the case of coke or charcoal.

G.

Patent Spring Folding Hat.

A new hat has lately been introduced into London from Edinburgh, to which it was imported from Paris, which is composed of a skeleton of highly tempered steel springs, extended by an internal metallic ring. It is covered with felt or silk, and may be formed into any fashionable shape, may be compressed into a very small compass, and instantly restored to its original form. *Open* and placed on the head, it resembles other hats, and is equally pleasant to wear; *shut*, it is reduced to the thickness of an inch and a half, when it may be carried under the arm, put between the coat and the vest, like a portfolio, and will only occupy the room of a shirt in a portmanteau. Its exceedingly simple mechanism may be adapted to various forms, while its durability is declared to be equal to that of any other hat: thus, for the convenience of travelling and attending crowded assemblies, this invention must be very useful. When the hat is to be put on, the metallic ring, which is covered with the lining, is pushed towards the inside of the hat; then the edges of the ring slide along the springs, forcing them and the outside covering to its proper form; in which, by means of a notch in the ring, it remains extended. When it is wished to compress the hat, two of the springs must be pressed with the thumb, which make the crown come down and reduce the hat to its compressed state. Mech. Mag.

Locomotives on Towing-Paths.

An experiment, strongly marking the onward spirit of our countrymen, was made on a Scotch canal, in the latter part of the month, which has just expired. Mr. T. Graham, the great patron of rapid travelling by canals, has induced a canal-company of which he is a member, to attempt the substitution of a locomotive engine for the horses by which their rapid boats were drawn. The experiment was made under the direction and personal superintendence of Mr. Macneil, the civil engineer. The result was of the most promising kind. The boats were drawn eight to ten miles under very unfavourable circumstances, and it was the opinion of several experienced practical men present, that there appeared no reason why eighteen miles should not be accomplished, when a little more practice has familiarized the apparatus, and the roads, &c., should be properly prepared to receive it.

Mag. Pop. Science.

Lunar Occultations for February 1838.

There are no occultations of stars to the sixth magnitude inclusive, visible in Philadelphia, in the month of February 1838.

Meteorological Observations for September, 1837.

Moon.	Days.	Therm. Sun rise.	Therm. 2 P.M.	Barometer. Sun rise.	Barometer. 2 P.M	Wind. Direction.	Wind. Force.	Water fallen in rain.	State of the weather, and Remarks.
				Inches	Inches			Inches.	
	1	50°	66°	29.86	29 95	W.	Moderate.		Clear—do.
	2	54	67	90	80	NE.	do.		Cloudy—Clear.
	3	56	69	90	30.00	W.	do.		Clear—do.
	4	50	69	30 20	15	W.	do.		Clear—do.
	5	52	70	30	30	W.	do.		Clear—partially cloudy.
	6	55	70	30	20	S.	do.		Clear—do.
☾	7	60	69	10	11	S.SW.	do.		Cloudy—do.
	8	63	70	29.90	29.90	W.	do.	1.35	Rain—do.
	9	60	72	90	95	W.	do.		Clear—do
	10	66	76	94	80	E.SE.	do.		Cloudy—do.
	11	69	78	80	70	W.	do.	.14	Cloudy—do.—rain.
	12	61	66	65	65	W.	Brisk.		Clear—flying clouds.
	13	48	66	85	90	NW.	Moderate.		Clear—do.
☺	14	46	66	95	30.00	E.	do.		Clear—do.
	15	51	65	30.05	5	N.	Brisk.		Clear—partially cloudy.
	16	49	62	5	6	NE.	Moderate.		Clear—cloudy.
	17	56	72	29.95	29.90	NE.	do.		Partially cloudy—clear.
	18	56	74	75	75	NE.SW.	do.		Clear—do.
	19	58	66	85	85	W.	Brisk.		Cloudy—clear.
	20	50	65	30.06	30.15	W.	do		Clear—do.
☽	21	47	64	20	25	W.SE.	do.		Clear—do.
	22	50	66	20	6	NE W.	do.		Partially cloudy—do. do.
	23	50	63	6	12	W.	do.		Clear—do.
	24	45	62	35	35	E.	Moderate		Clear—do.
	25	42	67	25	20	W.SW.	do.		Clear—do.
	26	55	74	5	00	W.	do.		Cloudy—clear.
	27	64	66	00	10	E.	do.		Cloudy—do.
	28	60	71	5	29.95	E.S.	do.	.35	Fog—cloudy—rain in night.
⊗	29	65	75	29.90	90	S.	do.		Fog—cloudy.
	30	54	62	90	80	E.	do.	.45	Rain—do.
	Mean	54.73	68.27	30.01	30.00			2.29	

	Thermometer.	Barometer.
Maximom height during the month	78.00 on 11th.	30.35 on 24th .
Minimum do.	42.00 on 25th	29.65 on 12th
Mean do.	61.50 .	30.01.

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